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SELECTED
ESSAYS AND ADDRESSES
BY
SIR JAMES PAGET

NEW EDITION (SEVENTH IMPRESSION).

MEMOIRS AND LETTERS OF
SIR JAMES PAGET,
BART., F.R.S., D.C.L.

EDITED BY STEPHEN PAGET,
ONE OF HIS SONS.

*With 6 Portraits (3 Photogravures) and 4 other
Illustrations.*

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P R E F A C E

THE lectures, essays, and addresses by Sir JAMES PAGET contained in this volume are collected and republished not as illustrations of his life, or as a book for 'general reading,' but for the special use of the medical profession. Of his lectures, those have been chosen that are most closely associated with his clinical studies, and with his later work in surgical pathology ; and to them are added some essays and addresses that will be welcome to men who knew him. None of his earlier lectures on pathology are included : there is, indeed, nothing more here than such a selection of Sir JAMES PAGET'S writings as any one of his pupils might wish to have made in honour of him and for love of him.

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SELECTED ESSAYS AND ADDRESSES.

I.

SENILE SCROFULA.

Published in the St. Bartholomew's Hospital Reports for 1867. The notes were added in 1875, when the essay was republished in Sir James Paget's 'Clinical Lectures and Essays.'

It is, I think, too often taken for granted that scrofula is almost exclusively a disease of the earlier part of life.¹ Doubtless, young persons are much more often the subjects of scrofula than are those of later years; but the old, *i.e.* people over 60, are, I believe, more often scrofulous than those between 30 and 50, and certainly are more often so than they are generally supposed to be.

The evidences of scrofula in the old are not only in certain diseases of internal organs to which a scrofulous origin may be probably assigned, but in the diseases of lymph-glands, bones, joints, the spine, the testicles, and other structures which appear to be the 'seats of election' of scrofula in the young. There is not one of these structures in which I have not seen, within the last few years, instances of scrofulous disease in people more than 60 years old. The cases appear equally frequent in private and in hospital practice, and no period of life is too far advanced for them; some of the most marked have been in patients above 75: one of them was in a patient 91 years old.

¹ The same error is in many minds respecting phthisis, even though many clear accounts of phthisis in the aged have been written.

The characters of scrofula in the old are, essentially, the same as in the young. I cannot, indeed, describe any striking peculiarities of form or features by which scrofulous old persons may be recognised ; but they are generally of weakly health, ready for disease, susceptible of it under comparatively slight provocation. They tell of weakly relatives, or of consumption in their families ; or of their having been subject to marked scrofulous affections in early life, and of having survived a time of great delicacy of health, and then passed into a comparatively healthy middle age.

In its local features, also, senile scrofula agrees with that of early life, or differs only in its yet greater slowness and the more thorough degeneracy of the affected parts. The same slow softening of textures ensues ; the same slow ulceration and slower healing, with pus containing granule-masses and granules rather than well-formed pus-cells.

But notwithstanding its likeness to so well-known a disease as the scrofula of young persons, the senile scrofula is sometimes difficult of diagnosis. In superficial parts, the skin and mucous membranes, the difficulty is usually between scrofulous affections and gouty. Between these, I believe that the diagnosis must be ensured by observing the co-existing constitutional characters, or other marks of disease. But this is often difficult : for gout and scrofula are often, by inheritance, so intermixed, that the resulting condition can hardly be analysed.

In deeper-seated parts, as the lymph-glands, bones, and joints, the difficulty of diagnosis is more frequently between scrofula and cancer. In the young this difficulty seldom occurs ; for in them cancer is comparatively rare in the parts in which scrofula is frequent. In the old, the lymph-glands and the bones are as frequently the seats of

scrofula as of primary cancer.¹ In the old, therefore, the difficulty of diagnosis between these two diseases may be greater and more frequent than in the young ; but I think that the difficulty is commonly due, in some measure, to our expecting to find cancer rather than scrofula in old persons. The regularly increasing frequency of cancer as age advances, *i.e.* its increasing frequency in proportion to the number of persons living at each advancing age, is well known : and this knowledge gives some prejudice in favour of believing that a swelling in an old person is very likely to be cancerous. Scrofula, on the other hand, is often presumed to be very unlikely. I believe that if it were generally known to be not very unlikely, the difficulty or hesitation in making out its existence would nearly cease.

Some general rules for the diagnosis, however, may be kept in mind.

In the cases of doubtful diseases of the bones or of doubtful swellings not glandular, the scrofulous affections are generally inflamed, however lowly ; the cancerous are not so. Tenderness and heat are indications of scrofula rather than of cancer, and are rarely absent at and about a scrofulous bone at whatever time of life ; with cancer they are present only when the diseased part is casually inflamed.

Spontaneous pain—that is, pain independent of movement or pressure—indicates, if severe, cancer rather than scrofula ; but it is a very fallacious sign, especially in the earlier periods of disease.

Redness over diseased bones tells of scrofula more than of cancer. When present with scrofulous disease in the old it is duskier and less ruddy than in the young ;

¹ Many cases here referred to as 'cancer' would now be generally named 'sarcoma.'

not only because of the less brightness and probably slower movement of the old blood, but because of the deeper colour of the epidermis which, with its brown pigmental degeneration, partially veils and shades the colour of the blood beneath it.

In the diagnosis between scrofulous and primary cancerous lymph-glands in the old, the chief things indicative of cancer are hardness or at least great firmness of substance, close-clustering, deep-seated attachments, pain, and quick increase. The opposites of these conditions, especially when tenderness and external redness are added to them, commonly signify scrofula. In the cervical lymph-glands, which, in old persons, are the most frequent seats of both scrofula and cancer, the lower glands are most frequently scrofulous, the upper ones cancerous; the soft primary cancerous disease is very rare in the old, and not so rare in the young; the soft scrofulous disease or simple hypertrophy which is frequent in the young is very rare in the old; the cancerous disease in the old is often secondary to some comparatively trivial primary disease; the scrofulous is very rarely so.

Respecting the treatment of senile scrofula little need be said. I believe that whatever is useful for scrofula in the young, is not indeed useless, but less useful in the old. As age advances, all medicines that act by increasing the activity of organic processes become, as it seems, less potent; and so iron and cod-liver oil and iodine have comparatively little influence. Even high mountain-air and sea-air become, as age advances, less invigorating, and the more so the more the old age is attended with infirmities that hinder active exercise. As age advances rest and warmth and good food become constantly more important than any kind of air.

II.

CASES THAT BONE-SETTERS CURE.

*Clinical lecture, published in the 'British Medical Journal,' 1867,
republished in 'Clinical Lectures and Essays,' 1875.*

AFTER systematic lectures on the chief injuries of the bones and joints, it may be useful if I try to enforce by particular illustrations some of the general principles that I stated ; and it may secure your attention if I use the form of speaking of the Cases that Bone-Setters cure. For few of you are likely to practise without having a bone-setter for a rival ; and, if he can cure a case which you have failed to cure, his fortune may be made and yours marred.

I believe that, in the large majority of cases, bone-setters treat injuries of joints, of whatever kind, with wrenching and other movements of them. The proceeding was described to me lately by a gentleman who had a well-marked fracture at the lower end of his radius. He had been to a distinguished bone-setter, who, with a glance at the wrist, said : ' You ha' put out your wrist, that's what you ha' done ; ' then violently stretched and moved the joint ; then said : ' Now you go and hold that under my pump,' and, after the cold douche, took his fee. The fracture, being none the better for this treatment, was, at a second visit a few days later, again wrenched, pumped upon, and paid for. But, this time, much pain and swelling followed ; and the patient had the wisdom

to call himself a fool, and to go to his usual medical attendant, who sent him to me.

Cases of this kind are of frequent occurrence. To the bone-setter, every injured joint is 'put out;' and the one method of cure is the wrench and the rough movements, by which it is said that the joint is 'put in' again.

Now, it would be of little use for us to estimate, even if it were possible, the quantity of mischief done by treatment such as this. It is more important to know and consider that it sometimes does good; that, by the practice of it, bone-setters live and are held in repute; and that their repute is, for the most part, founded on their occasionally curing a case which some good surgeon has failed to cure. For here, as in all similar affairs, one success brings more renown than a hundred failures or mischiefs bring disgrace. The patients who are cured never cease to boast of their wisdom in acting contrary to authorised advice. They who are damaged are ashamed of themselves and hold their tongues.

What, then, are the cases that bone-setters cure with their practice of wrenching?

First, of course, they have a certain number of real fractures and dislocations which they reduce, and of old ankyloses which they loosen. Of these, I need say nothing; for I believe there is nothing in their practice in these cases which is not as well or better done by regular surgical rules.

Next, there is a rare accident which a wrench may cure, and which, if you are not on your guard, you may fail to make out; namely, the slipping of a tendon. I have known the tendon of a peroneal muscle slip to the front of the outer malleolus; and a tendon slip with great pain at the radial side of the lower end of the

ulna; and in three instances I have known the extensor tendon of the middle finger slip over the heads of the metacarpal bone and first phalanx; and here, from our museum, is the long tendon of a biceps slipped from its groove. Of these accidents, the first two may be made out by feeling the displaced tendon and the gap where it should be; the third may be at least guessed at by the signs which Mr. Soden has pointed out in his case, related in the *Medico-Chirurgical Transactions*; ¹ the slight forward prominence of the head of the humerus, its drawing up under the acromion, and the pain at the lower end of the biceps on stretching it. As to this displacement, however, I doubt whether it would be ever so certainly made out as to be fairly reduced; the others, at the ankle and the finger, should be remedied by relaxing the slipped tendon as extremely as possible, and replacing it with lateral pressure and sudden stretching.

Some other tendons may slip, I believe, like these; the tendon of the popliteus appears very likely to do so; and I can hardly doubt that a bone-setter has occasionally done, unwittingly, a lucky trick, when, with wrenchings and twistings of a joint, he has made some dislodged tendon slip back to its place.

But there is a set of cases much more common than these, which may be cured with wrenching and rough movements; namely, the so-called internal derangements of joints. The knee-joint is by far the most frequent seat of this injury, whatever it is; but the like occurs in the lower jaw-joint; and I have known very similar signs of injury at the hip and elbow. The most marked sign is that, while the joint is being moved in some ordinary action, most often when the foot rests firmly on the ground, and the thigh is rotated outwards on the leg,

¹ Vol. xxiv. p. 212 et seq.

something is felt slipping or suddenly caught between the bones, and a great pain comes and the joint is locked. It will move in one direction, not in the opposite one: just like a hinge with a stone in it (as a patient described it to me). The locking of the joint, which is usually at moderate flexion, is soon followed by effusion of fluid into it, and other signs of more or less acute inflammation of the synovial membrane; and, if nothing be done, these last for some days, or even for some weeks, before, with subsidence of the inflammation, the joint gradually regains mobility.

Many of these symptoms are like those due to a loose piece of cartilage in a joint—a much rarer condition. But, with loose cartilages, joints are not, I think, often locked for any length of time; they are stopped with extreme pain when the cartilage gets between the bones, but it soon escapes, and they go again. In some of the cases of what I am calling locked joint, at the knee or lower jaw, it is probable that one of the interarticular cartilages slips and is nipped between the bones. We have, in the museum, a cast from a knee in which it is certain that this happened. But in some cases it seems more likely that a fold of synovial membrane, or a portion of capsule, or of synovial fringe, is caught and nipped. However we may explain the accident, it is one of those that may be cured by the bone-setters. Such movements as theirs are not always necessary; and none should be practised recklessly or without plan; but force may be requisite and, if used knowingly, will certainly set a locked joint right again.

Sometimes a patient learns for himself how to unlock his joint, and can do it gently, first, in the case of the knee, bending and then with slight rotation slowly stretching it. But he may need more force than he can

use for himself; and you may apply it better than a bone-setter can.

In the case of the knee, the 'lock' usually takes place with the joint moderately bent and the leg rotated outwards. You must unlock it by extremely bending the joint, then rotating the leg inwards, and then suddenly and forcibly extending it. In the same manner, for any other joint that appears to slip and lock, you must observe the direction in which the patient can easily move it, and the direction in which movement is impossible or very painful; then you must move it, first, extremely in the former direction, and, secondly, forcibly in the latter. The manœuvre is sometimes very painful; and the force required for success may be greatly augmented by muscular resistance. In either case, the use of ether or chloroform may ease both the patient and yourself.

A fourth set of cases that may be cured with wrenching, or other forcible movements, includes those in which injured joints are held stiff, or nearly stiff, by involuntary muscular action. You may meet with such cases in patients of any age; but they are most frequent among the young. Sometimes after well-treated fracture near a joint; sometimes after a sprain; sometimes when a joint has been hit hard—stiffness remains, which is due solely to muscular action; and this stiffness in some cases is constant, and in others ensues on slight attempts at motion.

Any joint, I believe, may be in this condition at any time after an injury. I have seen it at the elbow, shoulder, cervical spine, hip, knee, and ankle; in some instances a few hours after the injury, in some, several weeks. You may know this muscular kind of stiff joint by this, among other signs: that the stiffness is not a

dead block, as if by meeting of displaced bones, nor has rigid resistance, but yields a little, as if with the 'giving' of a firm elastic substance which instantly recoils. Besides, you may generally feel the muscles in action; not hard and vibrating as if with all their force, but firm, steady, and resisting. If, however, you have any doubt about the diagnosis, ether or chloroform will settle it. As soon as the patient becomes quite insensible, the muscles relax, and the previously stiff joint becomes freely movable.

Herein appears the best mode of cure. When the patient is insensible, move the joint quietly, and then confine it with splints in a posture opposed to that in which it was stiff. After a day or two, it may be moderately exercised, douched, and shampooed; but in the intervals of this treatment the joint should be confined with the splints, if it should appear to be becoming stiff again.

You may sometimes see another condition, very like this involuntary muscular rigidity of joints, in young children. If one of its limbs be hurt, a young child will sometimes hold the limb steadily in one position, and complain if it be moved. Thus, a child, whose thigh has been strained, will stand on the other leg, and keep the hurt thigh lifted up, as if for extreme disease of the hip-joint; or, for similar hurts, will, for even many days, keep its arms close to its side, or its elbow-joint steadily bent.

Perhaps, some of these cases are the same as those I last spoke of; but in many of them the muscular fixing of the part has seemed to me not involuntary. It is more like a trick, or an instinct of fright lest the part should be hurt again. Certainly, the muscles relax instantly in sleep, and not unfrequently when the attention is distracted from them.

I suppose that bone-setters would cure this state with their panaceal pulling; but, happily, they are allowed to have but little practice among children. Happily, I say, for children's joints are much more imperilled by violence than those of older patients; and you cannot be too cautious in concluding, when a child holds a joint fixed, that there is really no disease or serious injury.¹ All the evidence must be negative; and an oversight may be disastrous.

However, you need not use any kind of force in this kind of contraction in a child. If the part be only allowed a few days' rest, it will get well; unless, indeed, it be seriously damaged—in which case, you will have done well by avoiding all violence.

In another set of cases there is no doubt of the voluntary character of the muscular rigidity of a joint. You saw lately a girl in the Lawrence Ward who wilfully resisted all movements of a hip that had been only slightly hurt. If a bone-setter had wrenched her joint it might have served her right, and the pain might have cured her temper. But she recovered just as well when she saw that she did not deceive us and was not pitied. You may expect to find cases of joints wilfully held stiff among the worser sort of schoolboys, and they sham pain as well as stiffness. It is lucky for them when they can escape punishment or disgrace by lying and letting the bone-setter be believed, when he professes that he has 'put-in' their dislocations.

Now, among all these cases of muscular difficulty, there is a good harvest for bone-setters; and, without

¹ The condition here described is sometimes imitated when infantile paralysis begins with great pain in the paralysed part, or with its being very painful when moved; but I think that the diagnosis may in these cases be made by observing that the muscles of the limb which is paralysed, or is becoming so, do not resist extension.

doubt, their remedy, rough as it is, is often real. Yours may be as real, with much less violence ; and, with better diagnosis than they ever make, you may do none of the harm that they often do.

But there is a yet larger class of cases which bone-setters sometimes succeed in curing very quickly ; namely, ordinary sprains.

I cannot doubt that some recently sprained joints may be quickly cured, freed from pain, and restored to useful power, by gradually increased violence of rubbing and moving. This method of treatment has many times been introduced into regular surgery ; but it has never been generally adopted, or, I think, long practised by anyone. I suspect that it sometimes does no good, and sometimes does harm enough to disgust a prudent surgeon.

I believe that the best mode of applying this plan of treatment is, to begin by handling, rubbing, and pressing the sprained part and its neighbouring structures very gently. After doing this for fifteen or twenty minutes, the rubbing and pressing may be increased in hardness, and the joint may be more freely moved, especially in the direction opposite to that in which it was forced by the accident. Another quarter of an hour or more thus spent is to be followed by rougher proceedings of the same kind, till even severe pressure and wide and violent movements can be borne without pain ; and then, in an hour or so, the cure is deemed complete, or so nearly complete as to require only a slighter treatment of the same kind on the next day.

I cannot tell you in what kind or proportion of recent sprains you may employ this treatment ; indeed, I cannot advise you to use it at all, unless by way of trial in very healthy men. For I do not doubt that it will sometimes do harm ; and the greater quickness of cure which it may

achieve is not worth a risk, while we can always employ such safe, and not slow, means as the combined rest and support of the sprained parts which are given by strapping or the starched or plaster-of-Paris bandage. In short, this rough-rubbing and hard-pulling treatment of recent sprains seems to me one of those dangerous remedies which, though I believe in their occasional utility, I would rather not employ till I can discriminate the cases in which they will do good from those in which they will do harm.

Such discrimination, difficult as it may be among recent sprains, is not very difficult among old ones; that is, among cases in which the ill effects of sprains remain long uncured. It is among these cases that bone-setters, and especially those who combine rubbing and shampooing with their 'setting,' gain their chief repute, and not without some right.

Among 'old sprains,' you will find a strange variety of cases—chronically inflamed joints, each probably bearing the marks of the constitutional disease or unsoundness of its possessor, and loose joints, and slipping, and creaking, and weak, and irritable joints, and many more. To all these, mere bone-setting does harm, or no good; and rubbing and shampooing are of little, if any, use; indeed, to a really inflamed joint they would generally be mischievous. But among 'old sprains' are not a few cases in which a joint, after long treatment, remains or becomes habitually cold. It is generally stiffish and weak, sensitive, aching after movement, or in the evening or at night, sometimes swollen, puffy or œdematous, but not with an 'œdema calidum.' Whatever else it is, it is cold, or, at the most, not warmer than the healthy fellow-joint. Among these cold joints, bone-setters and rubbers gain, as I said, great repute; and all the more because they

often get the cases after the patients have become tired and discontented with a rather over-careful surgery. Admirable as is the rule of treating injured joints with rest, such rest may be too long continued; and in every case in which it has done full good, it must, in due time, be left off. With rest too long maintained, a joint becomes or remains stiff and weak and over-sensitive, even though there be no morbid process in it; and this mischief is increased if the joint has been too long bandaged, and still more if it has been treated with the cold douche.

I need hardly say that it may be sometimes difficult to decide the time at which rest, after having been highly beneficial, may become injurious; or that the decision is always a matter of grave importance. On the one hand, you and the patient may be losing time through over-caution; on the other, the risk may be incurred, through rashness, of renewing inflammation in a damaged joint. I believe you will be safe, if you will take the temperature of the part for your guidance. If the part be always over-warm, keep it quiet; if it be generally cold, or cool, it needs and will bear exercise and freedom from restraint of bandages, with friction and passive movements and other similar treatment of the reviving kind. And of this you may be the more sure when the cold integuments over the joint are dusky-pink or purplish, or become so when the limb hangs down, and when there is little swelling, and when pain is much greater than is accounted for by any appearance of disease.

I do not know whether bone-setters make any discrimination among these cases; and I do not advise you to adopt their rough method in all or in most cases; for though they may, when successful, prove emphatically the utility of movements for old sprains, yet the same

good may be more safely done with gentler means of the same kind. Exercise of the hurt part should be gradually increased and always followed by long repose; and the frictions and shampoos should be gradually made harder and more rough, and the passive movements gradually extended. Always, the part, if itself cold, should be, by any means except bandages, kept warm; and always the patient's constitutional defects should be watched and, if possible, amended; for very commonly the chief hindrance to the recovery of a sprain is not local, but some general wrong—gout, chronic rheumatism, or struma, or hysteria, as it is called.

An 'hysterical joint' is, indeed, sometimes a rare opportunity for a victory for a bone-setter. Cold, weak, useless for want of power of will, intensely sensitive, subject to all the seeming caprices of a disorderly spinal cord and too vivid brain—such a joint as this may be cured by the sheer audacity with which it is pulled about. If nothing in it but its portion of the nervous system is in fault, this may be sometimes cured through influence on the mind. And so not only bone-setters, but the workers with Mesmerism, and tractors, and oils, and distant or superficial electricity, can sometimes cure hysterical joints: for the patients love to be cured with a wonder; and the audacious confidence of all these conjurers is truly wonderful.

From all this, you may see that the cases that bone-setters may cure are not a few. I think it very probable that those in which they do harm are numerous; but the lessons which you may learn from their practice are plain and useful.

Many more cases of injured joints than are commonly supposed to be thus curable may be successfully treated with rough movements—wrenching, pulling, and twisting.

The cases that are thus curable I have endeavoured to point out to you. Be on the watch for them. But remember always that what may be treated violently may be treated more safely and as successfully with comparative gentleness; and that, in some cases, you may very advantageously use chloroform or ether. And remember, also, that no degree of violence, not even such movements or exercises as I have advised, can be generally safe in the treatment of injured joints, unless when directed with a skilful discernment of the appropriate cases.

Learn then to imitate what is good and avoid what is bad in the practice of bone-setters; and, if you would still further observe the rule, *Fas est ab hoste doceri*, which is in no calling wiser than in ours, learn next what you can from the practice of rubbers and plasterers: for these also know many clever tricks; and, if they had but educated brains to guide their strong and pliant hands, they might be most skilful curers of bad joints and of many other hindrances of locomotion.

Since the publication of this lecture, a valuable essay on bone-setting has been published by Dr. Wharton Hood,¹ who has thoroughly learned the art, and practises it skilfully. He fully describes the several methods of manipulation, and no one can doubt their value when used prudently.

My later experience, as well as Dr. Hood's essay, makes me believe that my account of the 'cases that bone-setters cure' is not very faulty. I have made some corrections in it and have some things to add.

Chiefly I would add emphasis to what I have said of the mischief of keeping injured joints too long at rest or

¹ *On Bone-Setting*, 1871.

too cold. Too long rest is, I believe, by far the most frequent cause of delayed recovery after injuries of joints in nearly all persons who are not of scrofulous constitution. In the healthy, the chronic-rheumatic, and the gouty, it is alike mischievous; and not only to injured joints, but to those that are kept at rest because parts near them have been injured. Mere long rest stiffens them, and makes them over-sensitive; cold douches and elastic restraints and pressures make them worse, and nothing remedies them but movements, whether forced or voluntary. The same may be said of joints which have been kept too long at rest for inflammations of whatever kind, still excepting the scrofulous, which should rarely be treated roughly, and never without much caution.

Dr. Hood's essay should be read on all these cases, not only for the manual treatment which he teaches, but for the signs which he indicates as decisive in the choice of cases. He believes that the success of 'bone-setting' in them is due to the rupture of adhesions; and this may be. I have seen such adhesions in the ankle-joints of legs amputated after being long at rest, though the joints had not been evidently inflamed; and Mr. Butlin¹ has related a case of ankylosis of a knee-joint in a limb which was long kept straight for the treatment of a fractured femur. But even without adhesions a joint long at rest may become restrained in certain movements by the gradual shortening of all those parts of its ligamentous structures which have been constantly relaxed, and it is likely to be very painful when a strain is put upon these shortened structures to restore them to their natural length.

Among the most frequent instances of painful stiffnesses induced by long rest of unhurt joints are those of the fingers after fractures of the fore-arm, and especially

¹ *Trans. Pathol. Soc.* vol. xxv. p. 212.

after fractures of the lower end of the radius. And this gives me occasion to say that, of the many inventions for treating this fracture, all are bad which hinder the easy movement of the phalanges of the thumb and fingers.

Another set of instances are those of the tarsus, which, though itself unhurt, may remain stiff and painful after diseases and injuries for which the leg has been long kept at rest. These are not met with, I believe, when the foot has been kept at a right angle with the leg, as in every such injury it should be. In this posture it will bear, without strain or pain, the weight of the body when walking is resumed.

Other similar groups of cases might be cited ; but they would all come under the general descriptions of joints becoming stiff and painful, or unable to bear the strain of ordinary movements, through being kept too long at rest. And if I should be asked how soon an injured joint may be safely moved or allowed to bear weight, I should say, generally, as soon as it is cool during any considerable part of the day, and not at every part tender on pressure, and not painful when its articular surfaces are pressed together. But particular judgments may have to be formed in the cases of the scrofulous, the 'hysterical,' and the gouty ; in the first, rest is seldom too long ; in the second, seldom too short ; in the third, the general health must be considered.

As to the methods of moving the joints thus stiffened and painful, it is sufficient, in a very large proportion of cases, if the patient will resolutely use them ; bearing some pain and not fearing it as if it were a sign of disease either present or impending. In the worst cases, violent movements must be used ; and I believe the best will be such as Dr. Hood describes. Ether or chloroform may be used, and will sometimes save violence by finding that

much of the rigidity is only muscular. But I must add that in all but the slightest cases, or the 'only nervous,' the forced movements are only the beginnings of cures. The popular belief, that the cure by bone-setting is complete at once, is erroneous; a good start is obtained with great *éclat*; but voluntary resolute exercises, or repeated forced movements, are necessary to maintain or increase the advantage first gained.

I have not mentioned in the lecture a set of cases which are said to be sometimes cured by bone-setters, and in which, after strains or other injuries of the spine, stiffness and aching long remain, and especially pain or tenderness over one spot at which, the patients sometimes tell, a crack or a slip was felt at the time of injury. I have seen several of these cases, chiefly in nervous and over-sensitive women and girls: bone-setting, I have heard, has cured some; in more, I know that it has failed; and these have slowly recovered without any evident influence of treatment.

On these and, indeed, on all the cases of which I have been speaking, I recommend the study of Dr. Hood's essay. It may enable any surgeon to do what I advised; 'to imitate what is good, and avoid what is bad in the practice of bone-setters.'

III.

ON STAMMERING WITH OTHER ORGANS THAN THOSE OF
SPEECH.

Contributed to the Physiological Section, at the Oxford meeting of the British Medical Association in 1868; published in the 'British Medical Journal,' 1868, and republished in 'Clinical Lectures and Essays,' 1875.

THE characters of stammering in speech are so well known, and may be so often studied, that we may take this form of disease as a type of a class including similar affections of other organs than those of speech, and may apply to all the same generic name of 'stammering.'

Stammering, in whatever organs, appears due to a want of concord between certain muscles that must contract for the expulsion of something, and others that must at the same time relax to permit the thing to be expelled. Ordinary stammerers cannot at the same time regulate the contraction of the muscles of expiration for the proper expulsion of air, and the relaxation of those of the glottis, or (in different cases) of the tongue or lips, for permitting the expulsion of the air while it is being made vocal and articulate. Numerous as are the varieties and modes of speech-stammering, this discord of muscles is in them all. Its dependence on the nervous system and the mind is in fact plain enough, in theory very difficult. Perhaps it may help the study of speech-stammering, if similar disorders be watched in other parts of the body; but at present I want only to point out the facts for their importance in our practice.

Stammering urinary organs are not rare ; and they may be known by observing, sometimes in the same person, the exact parallelism between the difficulty of expelling urine and that of expelling the air in the ordinary speech-stammering. The patient can often pass his urine without any trouble, especially at customary times and places ; and, when he does so, the stream is full and strong, and he has 'nothing the matter with him.' But he is in trouble if anyone is looking at him, or if he is hurried, as at a railway station. At other times he suffers all the distress that he might have with a very bad urethral stricture. He cannot pass a drop of urine ; or, after a few drops, there comes a painful check, and the more he strains, the less he passes ; and then complete retention may ensue and over-filling of the bladder. In these characters, the case may closely resemble one of the ordinary instances of so-called congestive stricture, in which rapid swelling of some part of the mucous membrane narrows or closes the part of the canal which is least capable of distension. But the circumstances in which the difficulty arises are, in the two cases, very different. The stammering with the bladder occurs in just the same conditions as the stammering speech. There are few stammerers in speech so bad but that they can talk or read fluently when they are alone or with those whom they are most familiar with, or when they are entirely thoughtless as to their manner of speaking. Their worst times are when with strangers, or with persons or in places that are associated in their minds with stammering. It is just so with the bladder and urethra. One patient told me that, although he could usually pass urine well, yet there was one person with whom nothing could induce him to walk, because once, when he was with her, he wanted to pass urine, retired, and failed.

His experience of the effects of association of thoughts made him sure that, if he were again in the same circumstances, the same distress would come on him more intensely. Another, a clergyman, always passed a catheter before going into his pulpit. He had often had nervous troubles with his bladder; and once or more, having felt a horrid need of passing urine while he was preaching, he found himself, at the end of his sermon, unable to pass any. He said he felt sure that, if he were to go into his pulpit without the assurance of an empty bladder which his catheter (a No. 12, passed easily) gave him, he should be pressed with the desire to pass urine, and then should have retention. As a speech-stammerer might be unable to utter a word, so would he be unable to pass a drop of urine. Again, another patient has described himself as driven to all kinds of devices to bring about the association of ideas or of actions with which he best succeeds in emptying his bladder. He must walk up and down his room, and stand or sit in some customary singular posture, and then be very careful not to direct his mind either too much or too little to what he has to do, and then to let the urine run as inconsiderately as he can.

I might add many more notes of the seeming caprices of the stammering bladder and urethra; but it may suffice to say that nearly all the phenomena of stammering speech find in them their parallel. In both, alike, there is usually a history of the difficulty having been felt even in early life; in both alike are observed the strong influences of habit and of association of ideas; the effects of transient changes in the vigour of the nervous system; the need of a justly and yet almost unconsciously measured exercise of the will, that it should be neither more nor less than enough; and the influence of distraction of mind. And equally in both classes of patients

may be noticed the coincident general sensitiveness of the nervous system, and the family relationship with persons who suffer various other forms of nervous disorder.

One or two differences may, however, be noticed between the urinary and the speaking organs, in their respective stammerings. The former cause more pain. The bladder, unable to expel its contents, becomes for a time the seat of the feelings of distress, tightness and urgent need of emptying, which are felt in more simply mechanical retention of urine; and it becomes more sensitive and more irritable, but probably rather through the constant and earnest attention of the mind than through any change in its own condition. In cases of long-continued urinary stammering, some of which began in very early life, and some of which I have known for many years, I have seen no indication of any supervening organic disease. After years of trouble, nothing appears wrong but the manner of action of the parts. But though, so far as I have yet seen, stammering does not produce structural disease of the urinary organs, yet, in many of their structural diseases, the urinary organs become very 'nervous'—that is, very sensitive and disorderly in their nervous systems; and, in this state, they imitate some of the faults of stammering. Thus in stricture, especially from congestion of the mucous membrane of the urethra, patients feel that a great part of the difficulty of passing urine is due to their inability to regulate and harmonise the urinary muscular acts. As a man said to me, 'If I could stop the straining, I could do it; but, as soon as ever I strain, the spasm comes-on.' By this he meant that he could not duly moderate the action of the expelling muscles; and that, as soon as these began to act too vehemently, those that close the urethra would act in

spite of him. Just so a stammerer sticks fast in speech ; and the faster, the more he strains. In similar likeness to stammerers, we may see that most patients with long-standing obstruction from stricture or diseased prostate, or whatsoever else, resort to habits or postures, or mere tricks, by which they may gain the advantage of association of ideas for aiding the successful use of their muscular power. Even without more disease of the urinary organs than the increased frequency of action of the bladder which usually comes with advanced years, nervous and sensitive men let themselves fall into many troubles and need to be taught how to teach their bladders the habits of regular and quiet action.

The treatment of stammering urinary organs has difficulties similar and equal to those of treating stammering speech. The patient must try to educate himself to a calm control of his muscular power ; and, on any occasion of failure, must get what help he can from such mental tricks as I have referred to. He should evade all risks of difficulty and all the conditions in which he has suffered his worst failures. He should do anything rather than fail to pass his urine. He should not always yield to the first impulse to pass it, but should try to regulate the actions of the bladder to certain fixed hours of the day. And especially he should learn to use a catheter, not only that he may thus relieve himself in case of absolute need, but that he may be free from the enervating dread of helpless retention. He should keep his whole economy, and chiefly the secretion of urine, in the healthiest state he can ; for, like all other stammering, or in even a greater degree than any other, that of the urinary organs is influenced by the condition of the general health.

The characters of stammering with the organs of

deglutition may generally be recognised by their likeness to those of urinary stammering. They have to be distinguished not only from the mechanical obstructions of the upper part of the œsophagus, whether from stricture, pouch, or other hindrance, but also from the difficulties of swallowing that depend upon paralysis, whether hysterical (so called) or senile or from progressive muscular atrophy. It is not necessary that I should try to point-out the diagnosis of stammering in deglutition from each of these diseases. The common ground of diagnosis from them all is in the predominant influence of mental association in the stammering, and its slight, if any, influence in any of the other difficulties. Sometimes swallowing is easy and unhindered ; at others very difficult, especially in company, or when the trouble is particularly inconvenient, or the mind too much set on it. Hence, some of the patients always dine alone, finding it easy to swallow when they know that they cannot be seen. Briefly, nearly all that has been said of stammering with the urinary muscles might be repeated, *mutatis mutandis*, of that with the muscles of deglutition, and both may be studied by their likeness to the ordinary speech-stammering, with which, indeed, they may be associated in the same person or in the same family. A man of forty with well-marked stammering in swallowing told me that he had stammered in speech during all his earlier life, but he now spoke smoothly.

I have not seen cases enough to be able to discriminate between the stammering and the spasmodic stricture of the œsophagus. I think they are different affections, but I need more opportunities of studying them. If anyone will investigate them, much help may be gained from the paper by the late Dr. Brinton in the *Lancet* for January 6, 1866, and from the remarkable case of fatal

spasmodic (or stammering ?) obstruction of the œsophagus recorded by Mr. Henry Power.¹

I believe that a disorder essentially similar to stammering may be traced among the cases of difficulty of defæcation not due to organic disease; but it requires more study than I have yet been able to devote to it.

¹ *Lancet*, 1866, vol. i. p. 252.

IV.

WHAT BECOMES OF MEDICAL STUDENTS.

Published in the St. Bartholomew's Hospital Reports for 1869.

It is said that, on entering the anatomical theatre for one of his Introductory Lectures, Mr. Abernethy looked round at the crowd of pupils and exclaimed, as if with painful doubt, 'God help you all! what will become of you?'

I am not aware that any attempt has hitherto been made to answer such a question. The grounds on which I venture an answer are in the knowledge of what became of a thousand of my pupils within fifteen years of their entrance at St. Bartholomew's Hospital. The number may suffice for the grounds of that degree of general belief which, in a matter of this kind, is as near an approach to knowledge as we are likely to attain. And I believe that what may be told of the pupils of St. Bartholomew's would hold true of those of all the metropolitan schools, for with us the varieties of students, according to difference of birth, wealth, and previous education, are collected, I believe, in very nearly the same proportions as would be found in all the other metropolitan schools together.

The pupils from whose careers the following notes are derived were among those who attended, either my Demonstrations of Morbid Anatomy between 1839 and 1843, or my Lectures on General and Morbid Anatomy and Physiology between 1843 and 1859. Of the former,

I kept no complete lists, but have the names of 95; of the latter, I have complete lists, containing 1,131 names. Of the total, 1,226, many have been quite lost sight of.¹ The careers of 1,000 are known either to myself or to Mr. Callender or Mr. Smith, or all of us; for we have worked together for this essay.

Of the thousand—

23	achieved distinguished success.
66	„ considerable success. ²
507	„ fair success. ³
124	„ very limited success.
56	failed entirely.
96	left the profession.
87	died within twelve years of commencing practice.
41	died during pupilage.

In this table, they are classed as having achieved distinguished success who, within fifteen years⁴ after entering, gained, and to the end of the time maintained, leading practices in counties or very large towns, or held important public offices, or became medical officers of large hospitals, or teachers in great schools, as the Professors of Anatomy in Oxford, Cambridge, and Edinburgh, all of whom it was my singular good fortune to have for pupils.

Considerable success is ascribed to those who gained and still hold high positions in the public services, or

¹ Since writing the paper I have heard of a few more; but I have not used them for the tables. They would not disturb the proportions, which are more easily calculated in the exact thousand, than in a rather larger number. If it were possible to learn what has become of all those whom we have lost sight of, they would probably be classed in due proportions under all the headings in the table except the first two.

² Including 3 dentists.

³ Including 7 dentists.

⁴ Or less, in the cases of those entering between 1854 and 1859.

leading practices in good districts, or who retired with money earned in practice, or gained much more than ordinary esteem and influence in society.

The fair or moderate success which was the lot of rather more than half those whose histories are known, was that measure of well-doing which consisted in having a fair practice—enough to live with—maintaining a good professional and personal reputation, or in holding ordinary appointments in the public services, or in the colonies, and gaining promotion in due course of time.

Very limited success is assigned to those who, within the fifteen years, were not even in moderately good practice, or apparently likely to attain it; or who were just living, and that not well, by their work; or still employed as assistants in ordinary practices, or erratic and never prosperous; or doing much less than, with their education and other opportunities of success, they should have achieved.

They who failed entirely were a very mixed class, agreeing only in their total want of success. Of the 56 who made up the gloomy total, 15 were never able to pass examinations—some because of idleness or listlessness, a very few through sheer want of intellect. Of those who did pass, 5 failed because of scandalous misconduct; 10 through ill-health or misadventure, sheer ill-luck as it seemed; and 10 through their continuance in the same habits of intemperance or dissipation as had made us, even while they were students, anticipate their failure. Of the remaining 16 we only know that they have failed; they are not in disrepute, but they are barely maintaining themselves.

It will seem strange to everyone, I think, that so many as 96, that is, nearly 10 per cent. of the whole number, left the profession after beginning either its

study or its practice; and it is even less flattering to our calling that, to set over against those who left us, there were only 7 who came to us from other studies or pursuits in life, and 5 of these again changed their minds and never engaged in practice.

Of these 96, 13 while pupils left or were expelled in disgrace, and 3 were wisely removed by their friends. Of the remaining 80, 1 while still a pupil, and 1 after beginning practice, retired on private means, too rich to need to work; 4, after beginning practice, had to leave in disgrace—1 of these was rather sinned against than sinning; another, who had been a good student, speculated in mines, lost money, forged, and is in prison; 3 became actors—of whom 2 are in obscurity, and 1 is well esteemed in genteel comedy; 4 entered the army with commissions, 1 after and 3 before obtaining a diploma for practice; 3 pupils enlisted as privates, and 1 of these distinguished himself by courage and good conduct sufficiently to win a commission; 1, while a pupil, left for the Bar, and has succeeded; 5, after passing, took orders in the Church of England, 2 in the Church of Rome; 10 pupils, and as many after having begun practice, left for different forms of mercantile life at home or in the colonies; 3 pupils and 6 young practitioners took to farming. The remaining 27 left the profession for various pursuits, which need not be specified, unless to say that 3 became homœopathic practitioners, but took to that class no repute for either wisdom or working power.

On the whole, looking over the list and remembering the characters of those who left the profession for other pursuits, there appears no reason for believing that they have 'bettered' themselves. Some have succeeded, some have failed; the result would have been, I think, the same if they had remained in their first calling.

Last comes the melancholy list of deaths, telling that of those who entered nearly 13 per cent. were dead within fifteen years.¹ Of these 41 died while yet pupils, including 17 who died of phthisis, 4 (at least) of fever caught in the Hospital, and 2 who committed suicide; 87 died after beginning practice, some after attaining great success, some after long and vainly struggling in ill-health; 21 died of diseases incurred in their duties; 5 committed suicide, 2 of them under circumstances of great disgrace; 1 was hung, the notorious Palmer, who committed murder at Rugeley—he was an idle, dissipated student, cursed with more money than he had either the wisdom or the virtue to use well.

This, then, is what became of a thousand medical students; and, probably, the same lots, or nearly the same, in life have fallen or will fall to as many thousands more. It would be interesting if, with facts such as these, one could compare our profession with others, as to the chances and degrees of success that it offers to its students. But I know no facts that would serve for a comparison; nor would any be fair unless account were taken of the several amounts of capital in time or money expended upon each pursuit, and the times of reaching and the securities of retaining success in each, and their various social advantages and happinesses. On all these points we are without knowledge.

There might seem more hope of being able to tell the influence of different modes of education on the afterlife of medical students; and thence of deducing some scheme that should greatly increase the successes and decrease

¹ The number agrees so nearly with the general average mortality that it gives no reason for considering the medical profession either less or more healthy than other pursuits, at least in its earlier stages. For according to the 'English Life Table No. 3,' out of any 1,000 males who have attained the age of 19 years, 131 will die within 15 years.

the failures. But to do this with accuracy would require many more facts than anyone is likely to obtain. Of course in watching and reflecting on the careers of my pupils, I have come to some strong beliefs on subjects of medical education; but this is not the place for publishing them. Only one I will set down, which may be of use to future pupils, and is justified by some hundreds of personal recollections. In remembering those with whom I was year after year associated, and whom it was my duty to study, nothing appears more certain than that the personal character, the very nature, the will, of each student had far greater force in determining his career than any helps or hindrances whatever. All my recollections would lead me to tell that every student may draw from his daily life a very likely forecast of his life in practice, for it will depend on himself a hundredfold more than on circumstances. The time and the place, the work to be done, and its responsibilities, will change; but the man will be the same, except in so far as he may change himself.

V.

SEXUAL HYPOCHONDRIASIS.

Clinical Lecture, 1870, published in 'Clinical Lectures and Essays,' 1875.

THE cases which I advise you to include under this name are those of male patients who regard trivial maladies, or even some of the natural events, in their sexual organs with the unreasonable dread or gloom and watchfulness which are characteristic of hypochondriasis. They are such as are accused or accuse themselves of spermatorrhœa, or of the other maladies of the sexual organs which swindling advertisers profess to cure.

You will find that men with healthy nervous systems, or who are careless, or sensible, or well-informed, will very rarely consult you on any of the so-called functional diseases of their sexual organs; when they have them they endure them without harm or distress. Of those who will consult you, some are merely ignorant of what the natural actions of these organs are or may be; some have brains too emotional, or spinal marrows too irritable, hurrying the secretion of seminal fluids and disordering their emission; but those are most numerous whose minds, in reference to their sexual organs, are unsound. The unsoundness may not be sufficient to be called insanity: let it be called hypochondriasis; and if you will study its general characters with the help of the best essays on the subject, and best of all with the essay on Hypochondriasis by Sir William Gull and Dr. Anstie in

Reynolds's *System of Medicine* I may limit myself to speaking of the conditions of the sexual organs which are associated with disorders of the nervous system.

Of these I will speak rather fully ; but first, let me ask you to note the ignorance concerning sexual matters of which I just now spoke : for it is the source of a kind of hypochondriasis in some who, in mere ignorance, imagine miseries for themselves or are made miserable by others' falsehoods.

Ignorance about sexual affairs seems to be a notable characteristic of the more civilised part of the human race. Brutes, even those most changed by our domestication, copulate as naturally as they eat or defæcate. As the instinct for food leads them to eat, and carries with it all the knowledge necessary for the choice and taking of their food, so the sexual instinct has with it the knowledge how to copulate. It is the same, I believe, with the least civilised of our race ; but it is not so with the most civilised. It seems as if, in the course of generations, the transmission of intellectual powers gained by education had the effect of subduing or superseding those of instinct. How far up the grades of civilisation this change begins I do not know ; but among ourselves it is certain that the method of copulating needs to be taught, and that they to whom it is not taught remain quite ignorant about it ; as ignorant as, I suppose, we should be of what to eat and drink if we were not taught. Of course very few, I mean very few of our sex, grow up without being taught, either by the talk of schoolfellows or by books or other means ; but a few grow-up and even marry in complete ignorance ; and this ignorance, which is rare among men, is very common among well-educated women.

The fact is of much interest in relation both to the

natural history of our race, and to the frequency of sexual disorders dependent on the mind or on the nervous system. For sexual desire arises and grows without the knowledge how to satisfy it; and in the learning how to satisfy it errors and fancies and things half understood get into the mind, and become to some men sources of misery and fright, and to some the subjects of hypochondriac gloom and watchfulness.

Among the merely ignorant you will find that, if they be otherwise sensible people, they need only to be told the truth concerning the disorders, real or imaginary, for which they consult you. Knowledge will cure them. But if they be or have become hypochondriac they will not receive, or will not retain, knowledge; their erroneous beliefs will be to their minds stronger than your truths.

Of these I shall have to speak again in reference to all the sexual disturbances of which they complain.

Now the complaint of some is that semen passes with their urine, and that all their 'strength is going from them'; and of this they give various symptoms, which, if they be at all true, are due to something else. For the general notion of semen passing with the urine is erroneous and is usually derived from dishonest advertisers, who make it one of the grounds on which they rob their patients.

There is, indeed, a very rare case in which after gonorrhœa or other disease affecting a seminal vesicle, a part of it seems to become sacculated and may be so filled that mucus, and perhaps seminal fluid, may be pressed from it in the last lifting efforts of the muscles for expelling urine. And this out-pressing may be attended with sensations of discomfort, shuddering and tremors; but it is harmless. In the ordinary cases, and those of hypochondriasis, the supposed semen is mucus from the bladder which, when it exists in its natural small quantity

in the urine, appears as a pale dim cloud at the bottom of the vessel, sometimes sparkling a little when light passes through it.

As to semen passing with the urine, I am nearly certain that it never does so unless when an emission of semen, in whatever way provoked, has lately taken place, or where there has been disease of a seminal vesicle. In the former case some semen, remaining on the walls of the urethra or possibly having passed into the bladder, is washed out with the next stream of urine, and may be found in it with the microscope. I once examined, for many days in succession, the urine of a patient who was persuaded that he passed semen with it; and semen could always be found when he had had a nocturnal emission, but never on any other occasion. A former colleague of mine assured me that he had frequently observed the same thing after copulation. And this, I believe, is the whole truth concerning semen passing with urine; whatever may chance to be left in the urethra after an emission is washed out. But that which frightens the ignorant and the hypochondriacal is not even this; it is mucus of the urinary passages, either quite healthy or in some trivial manner changed. This form of what is called spermatorrhœa, therefore, should be treated by instruction, which the merely ignorant will receive and the hypochondriacal, very probably, will not.

Not much unlike this misunderstanding about vesical mucus is another in which some people, chiefly middle-aged and elderly with diminishing sexual powers, make themselves miserable. They find in their urine little flakes or threads of floating mucus which, they say, are always washed out of the urethra at the beginning of the urine-stream, especially in the morning. They watch these with the greatest anxiety; and send them to you on

bits of paper or of glass, begging you to examine them very carefully. I believe that they are bits of prostatic mucus, secreted in the night and washed-out with the morning urine. But, whatever they may be, they are not of the least importance. You may find them passed by men who neither know nor care anything about them, and whom they never harm ; and even hypochondriacs go on, month after month, passing them and yet suffering nothing but their mental misery.

There is really nothing more serious in these flakes of mucus, than there is in the mucus for which some men, or even these same men, blow their noses oftener than others ; or in the secretion which makes some people's eyelids stick together in the morning. It seems strange that mucus from one place should make people so much unhappier than mucus from another. But this is the usual character of hypochondriasis ; it is as if a morbid element of the mind could localise itself, as a morbid element of the blood may, in some 'place of election.'¹

Again, some or the same persons are unhappy because, as they say, they pass semen during defæcation. But these, again, do nothing more than healthy men often do. When the rectum is emptied with much muscular force, and especially when large solid fæces are being passed, the contents of the vesiculæ seminales and of the prostatic ducts and, I dare say, of Cowper's and other mucous glands besides, are apt to be pressed-out ; and hence it is not rare for healthy men to find mucus, or some fluid like it, escaping from the urethra during defæcation attended with straining. And when the vesiculæ seminales are

¹ Prostatorrhœa, prostatic gleet, or whatever else the disease may be called, which is attended with constant excessive secretion from the prostate or vesiculæ, or both, is not here referred to. This is a real disease and very troublesome, whether associated or not with spinal irritation or with hypochondriasis.

filled with semen, as they may be when it is long since an emission took place or when, in an emission, they are not emptied, their fluid together with semen may be pressed from the urethra with something even of the sensation of emission. All these things happen to healthy people without harm: they are not unnatural; certainly they are not effects or signs of disease: and when anyone comes to you complaining of them, it is his mind, not his sexual organs, that requires treatment.

Another subject of gloom and alarm to some is that, during sexual excitement and, as they suppose, worse still, when they wake in the morning, they find a clear colourless fluid flowing from the urethra or easily pressed from it. Here, again, the complaint is of that which is natural, and it would be quite as just if directed against tears during grief. The urethra naturally secretes mucus during sexual excitement; it secretes more or less in different persons, but some, I believe, in all; and as for the morning secretion, it is due either to some sexual excitement during sleep, forgotten before waking, or to the general condition of turgescence or erection of the sexual organs which, in most healthy persons, exists during sleep or some part of it. In no case is this clear urethral mucus a sign or consequence of disease, unless indeed when an excess of it is a residue of gonorrhœa. It is, I think, most abundant and most quickly formed in those whose sexual organs are more irritable than potent; but this is the worst that can be said of it; and even in these it is not the sexual organs, but some part of the nervous system, the brain or the spinal marrow, that is in the wrong. In no case does the secretion deserve to be called or treated as a disease.

Again, there are some to whom, whether through ignorance, or misguidance, or hypochondriasis, a varicocele

is a source of misery and dismay. They look on it as a forerunner of impotence, and of wasting testicles, and I know not what besides. All such fears are groundless. Varicocele is troublesome because of the sense of weight and aching which sometimes, though far from always, attends it, and which is sometimes much increased by long standing or walking. In some cases, too, the dilated veins, like varicose veins in the leg, are apt to become inflamed or very sensitive. But this, I believe, is the widest limit of the harm that varicocele ever does. I do not believe that it ever produced wasting of a testicle, or impotence, or any such thing. It is common enough to find varicocele in quite healthy men who, being sufficiently careless or sensible to make light of it, suffer no harm either mental or bodily. Some who have it while they are single and chaste are cured by marriage; and in some it ceases to cause even its slight occasional aching when they begin to grow old. In short, the cases in which varicocele is more than a trivial affair are very few; and in these few its mischiefs are not such as the sexual hypochondriacs imagine. They are, indeed, altogether distinct from the functions of the sexual organs; being such achings and wearing pains as may be felt in varicose veins in the legs. These may be sufficient to disqualify a man for military service; but they are not sexually important; and, in those who believe they are, it is a mental error, not a bodily one, that needs cure.

I can very positively give you the same assurance about those who will consult you on several other things; such as the scrotum being too pendulous, or the penis being cold or shrivelled, or the testicles too small when in fact they are of ordinary size.

But now I must speak of something which may, in its greater degrees, have the character of real if not serious

disease; I mean the nocturnal and other involuntary emissions of semen. I say, in its greater degrees; for, in the lesser, the emissions are natural, and it is a sign of ignorance or hypochondriasis if the mind dwells sadly on them. And I may add, that of all the cases of such emissions on which you will be consulted, not more than one in fifty will deserve serious consideration unless for the state of the patient's mind and nervous system.

To those who lead chaste lives, and to some of those who do not, nocturnal emissions of semen are natural occurrences. I never met with a chaste healthy man, of whom I had occasion to ask about them, who did not say that he had them sometimes. Their frequency is in different men very various: varying according to many things, such as climate, diet, social habits, and, above all, I think, according to the degree in which the minds of those who do not have sexual intercourse are directed to sexual matters. Thus nocturnal emissions may vary from once or twice in a week to once in two or three months, or, at times of unusual exhaustion or excitement, they may exceed those rates or may fall short of them: but in both sets of cases and in all the intermediate frequencies they are consistent with good health. Men who are careless, or sufficiently well-informed, say nothing about them and suffer no harm. When, therefore, any person with a sound nervous system, and having nocturnal emissions within such limits as I have mentioned, consults you about them, it is best to tell him that they are natural occurrences which may be left to their own course, and you may add that they cannot be put an end to and ought not to be if they could.

But the case is different in men with over-sensitive nervous systems, or in whom one may believe that at least that part of the spinal marrow which is in nearest relation

with the sexual organs is over-irritable. In this condition the emission of semen is apt to take place with much less than the normal amount of excitement. Hence it may take place too quickly during or even before sexual intercourse; sometimes without erection and almost without sensation; sometimes from the mere friction of the dress in riding or walking, or during sensual thoughts; and frequently at night with or without sensual dreams. This, if anything, might be called spermatorrhœa; but even this is not properly a disease of the sexual organs: it is a disease or a disorder of the nervous system, and may most probably be referred, as I have said, to a too irritable condition of the spinal marrow or of some portion of it. For, with the too frequent and too quick emissions there are always other signs of nervous disorder which, though commonly regarded as due to the emissions, are really not so.

The chief of these signs are aching of the back and lower limbs, especially after emissions; readiness to be fatigued, and in all fatigue pain; weary limbs and spines; indisposition or seeming incapacity for mental exercise; defect of will and of power of attention; often restlessness at night and unrefreshing sleep; hysteric fits or feelings. Many patients, too, are troubled with palpitation; many with constipation; some with excess of lithates; some with oxalates in their urine; not a few with irritable bladder; some with various nervous indigestions; some with coldness of feet and hands.

Now, neither these nor any other of the signs commonly enumerated with them are characteristic of disease of sexual organs: they are signs of a central nervous disorder; they are the very same as are found in many cases of 'spinal irritation' and of so-called hysteria in which there is no indication of any sexual disturbance, but, if

anything, some disturbance at the heart, or the bladder, or a joint or some other part. Moreover, these nervous signs bear no proportion to the emissions, and, though commonly aggravated by them, are only so aggravated as the same symptoms are in nervous women at the menstrual period, or by any other sudden or considerable expenditure of nervous power.¹

The utter prostration which women, and some men too, with spinal irritation complain of after walking is the counterpart of that which is complained of by these nervous patients after their emissions; and I have heard patients complain of it even during digestion, or after their daily defæcations, though these were with neither pain nor straining. It is true that there are few if any of these cases worse than those in which the nervous disorder, which I hold to be the primary affection, is directed on the sexual organs, or has been wilfully directed on them by frequent masturbation or irregular sexual practices; but, in not a few of those who lay the fault of their nervous troubles on their seminal emissions, the sexual organs act healthily. One of the worst cases that I have ever seen was in a married man who, because of backache and many more of the sensations I have enumerated, lay, like many hysterical women, constantly on his back, travelled on his couch, or at most moved slowly on crutches. He had occasional sexual intercourse; and his back was always more painful after it, and he felt miserable and exhausted and prostrate; but so he did after a walk or any other unusual expenditure of nerve-

¹ This lecture was given before those on Nervous Mimicry; but the points of likeness between the subjects of sexual hypochondriasis and those with disorderly nervous systems to whom those lectures relate are very numerous and clear. Especially, there is likeness between the men with this disease and the women who have uterine disturbance: and the difference in their mental states agrees with the general fact that hypochondriasis is much commoner in men than in women.

force. In another, a man of 30, who lay helpless and will-less, weak-eyed and utterly enfeebled, a very type of the supposed victims of spermatorrhœa, nocturnal emissions occurred very rarely. They were followed by increase of backache and other miseries, but in no greater degree than was every unusual mental or bodily effort : and this patient had never had sexual intercourse, had masturbated only twice or three times in his life, and had had very few nocturnal emissions.

Now, in some of these cases of rapid and frequent emissions, the consequence, as I want you to believe, and not the cause, of nervous disorder, there is no mental error : the patients are not more than reasonably distressed by the inconvenience they suffer. But in many cases hypochondriasis is associated with the rest of the disorder, and increases immeasurably both the misery and the difficulty of cure. The patients are full of apprehensions, unable to divert their minds from their sexual functions, constantly watchful of their sensations and making them constantly more intense. And further mischief follows all this : for the direction of the mind to the sexual organs makes both them and the parts of the nervous system associated with them more and more irritable ; it increases the secretion of seminal fluid, and hurries its discharge. The mind thus continually multiplies the sources of its own misery.

Few conditions are more pitiable than those of hypochondriacs who thus suffer, and few more difficult to cure. Your chance of doing good will depend mainly on the skill with which you can influence the patient's mind : for of the components of his case the mental condition is the worst, the irritable spinal marrow the next, the state of the sexual organs the last, in order of gravity. Not that local treatment is to be neglected, for if the unnatural

sensibility of the sexual organs can be diminished the mind may be less often distressed by emissions.

To this end, cold enemata are sometimes useful, and sometimes galvanism; and in some cases the passing of bougies or catheters, with or without caustic for the prostatic part of the urethra. But all these things often fail; and, as for the last, I have seen so many cases in which it has been mischievous that I am sure it should be used seldom, and never without more than ordinary skill. But of its being sometimes useful under these conditions I cannot doubt.

For the nervous state you must use, as for any other cases of 'spinal irritation,' iron, good food, and good air, and the correctives of any coincident disorder of the digestive or other organs; and you must persuade to a robust, sensible, and fully occupied habit of life, with much sleep and the best self-control that can be obtained.

But do what you may the hypochondriasis will, in the worse cases, remain; and if, even, the sexual trouble should cease, the mental wrong will continue, only changing its subject, or dwelling on the past as gloomily as it used to dwell on what was present. And in some the hypochondriasis will gradually drift into a more evident insanity.

Now, when one of these patients becomes insane the blame is commonly laid on his sexual organs, or on his having practised masturbation. Before I end, I will tell you the wrong of this; but I must first speak of another of the conditions on which hypochondriacs think erroneously: namely, impotence. This impotence, or even greatly reduced sexual power, is so distressing even to those who may be called reasonable men, that you may sometimes be ready to ascribe to mental disorder what is

a material disease or defect. Your study, therefore, in each case, must be to ascertain whether the impotence complete or incomplete be real, or due to ignorance or some nervous disorder; or whether there be no impotence at all but only a hypochondriacal fear or false persuasion of it. Now of the real cases of impotence I can only enumerate the chief forms. It may be due to disease or wasting of the testicles, but this, unless the disease or wasting be extreme, is very rare. (Observe, I do not speak of sterility which means only inability to beget children, but of impotence which means inability to copulate.) It may follow abscess or other acute disease of the prostate. Very rarely and inexplicably, it is sometimes a sequence of fever; sometimes of injuries of the brain or spinal cord. It is found during exhaustion from excessive and anxious mental work, with 'jaded brains'; and during some forms of dyspepsia, with oxaluria: but in these cases it is only temporary. It is not rare with advanced diabetes; and is common with several forms of degeneracy of the spinal cord. In old age it is happily not rare. At any age it may begin and continue very long in those who have been excessive in either sexual intercourse or masturbation. Nay, all sexual power and desire may cease in apparently healthy men, and without apparent cause, at unusually early ages: in cases that I have known, as early as thirty-five or forty, even in those who never masturbated and very rarely had sexual intercourse.

But although the physical causes of impotence and great decrease of sexual power may be thus numerous, yet from all these causes together the cases are less frequent than those due to nervous disorder or to mental defects; and the impotence which is complained of or dreaded without any real reason is more common still.

The mental and nervous defects which may make a man impotent are various in different persons; some hindering or interrupting erection; some preventing emission; and they are as various in degree; some are only occasional, a few are habitual or scarcely constant. They may be cured, if at all, by means addressed to the mind or to the nervous system; but they are all hard to cure; as hard as it is to cure stammering, whether in speech or any other function, or to cure any of the disorders of those functions for the perfection of which the will must act in exact harmony with parts not under its direct control.

I have enumerated all these causes of impotence to help you to guard against the risk of treating as a mere hypochondriac anyone who really has this malady from either mental or physical causes. As a rule the distinction is not difficult. They who complain of impotence alone are distressed about it, and very anxious for its cure; more so than to the cool judgment of anybody else may seem reasonable; but here is the boundary of their unhappiness; they do not tell or prophesy other miseries, and do not give up their minds to their diseases. Moreover, they who are impotent, or nearly so, from other than mental or nervous states have a loss of sexual desire as well as of power.

The sexual hypochondriac may or may not be mentally impotent; but in the great majority of cases is not. Most of those who consult you will tell you that though they have sexual desire yet they are impotent, or are afraid they are, and are therefore afraid to marry, because they have some of the trivial things I have been speaking of; occasional nocturnal emissions, or urethral mucus or varicocele or something not more important. Now if a man has sexual organs, including the

prostate, not manifestly diseased or wasted, and has erections and occasional nocturnal emissions, and any sexual desire, you may be sure that he is not impotent, unless he has very clear facts to prove that he is. The statements that hypochondriacs make to show that they are, or are becoming, impotent are usually evidences that they are not. And what is true of hypochondriacs is equally true of those who are frightened by mere ignorance of sexual matters or who have been fraudulently misinformed.

You may observe that, in speaking of sexual hypochondriasis, I have spoken of three different classes of men or boys in whom functional disorders of the sexual organs may need to be treated. There are, first, the merely ignorant or misinformed; next, those with over-sensitive or too irritable nervous systems; and lastly, the hypochondriacs. The conditions respectively characteristic of each may be mingled in various degrees, but they are worth keeping in mind as guides to treatment. The patients of the second class alone need medicinal help, and what this may be I said just now: the others must be mentally helped.

With careful and very positive teaching you will cure the ignorant, and do good to all but those whose hypochondriasis is near to complete insanity. But on some subjects of your teaching you will have to be very clear as to matters of fact; especially, for instance, as to the practice of masturbation, to which many of your patients will ascribe their chief distresses.

Now, I believe you may teach positively that masturbation does neither more nor less harm than sexual intercourse practised with the same frequency in the same conditions of general health and age and circumstance. Practised frequently by the very young, that is, at any time before or at the beginning of puberty, masturbation is very

likely to produce exhaustion, effeminacy, over-sensitiveness and nervousness ; just as equally frequent copulation at the same age would probably produce them. Or, practised every day, or many times a day, at any age, either masturbation or copulation is likely to produce similar mischiefs or greater. And the mischiefs are especially likely or nearly sure to happen, and to be greatest, if the excesses are practised by those who, by inheritance or circumstances, are liable to any nervous disease,—to ‘spinal irritation,’ epilepsy, insanity, or any other. But the mischiefs are due to the quantity, not to the method, of the excesses ; and the quantity is to be estimated in relation to age and the power of the nervous system. I have seen as numerous and as great evils consequent on excessive sexual intercourse as on excessive masturbation : but I have not seen or heard anything to make me believe that occasional masturbation has any other effects on one who practises it than has occasional sexual intercourse, nor anything justifying the dread with which sexual hypochondriacs regard the having occasionally practised it. I wish that I could say something worse of so nasty a practice ; an uncleanness, a filthiness forbidden by God, an unmanliness despised by men.

Another point on which you may have to teach is that of dreams associated with nocturnal seminal emissions. Men of scrupulous conscience are deeply distressed with the thought that these emissions are due to sexual feelings which they ought to be able to suppress even in their dreams ; they look on them as tokens of a prevalent impurity of mind which they must cure. Well, you may tell them that, according to all we know of dreams, it is not the dream that excites the emission, but the natural and involuntary erection and emission that determine the dream, and that over the erection and emission that may

occur in sleep or on just waking it is impossible that any man should exercise direct control ; he might as well try to control while asleep the tone of his snoring or the posture of his limbs. Some indirect control a man may have on all these things, and of the sexual part of them it may be held that the more the mind while awake is occupied in other than sexual matters, and so occupied that it is not even necessary to use any effort for the suppression or exclusion of sexual thoughts, the less will be the secretion of semen and the sensibility of the sexual organs, and therefore the less frequent the excitements and emissions during sleep. But, in some persons and, as I believe, in the great majority of those who are chaste, nocturnal emissions and the associated unclean dreams are simply irrepressible : they are due to a natural secretion of semen which we have no means of suppressing and no right to suppress. Therefore, to men with healthy nervous systems you must tell that their nocturnal emissions are evidences of health rather than of disease. And to those in whom too frequent emissions are connected with a too irritable state of the spinal marrow, you may tell that they cannot and ought not to be wholly suppressed ; but that they may be remedied by marriage and may, very probably, be diminished by means that will improve the condition of the spinal marrow.

To all alike you may try to teach a virtuous and judicious carelessness about these things ; a state of mind which would be an inestimable blessing to many besides these sexual hypochondriacs.

Many of your patients will ask you about sexual intercourse and some will expect you to prescribe fornication. I would just as soon prescribe theft or lying or anything else that God has forbidden. If men will practise fornication or uncleanness it must be of their own

choice and on their sole responsibility. We are not to advise that which is morally wrong, even if we have some reason to think that a patient's health would be better for the wrong-doing. But in the cases before us, and I can imagine none in which I should think differently, there is not ground enough for so much as raising a question about wrong-doing. Chastity does no harm to mind or body ; its discipline is excellent : marriage can be safely waited for ; and among the many nervous and hypochondriacal patients who have talked to me about fornication, I have never heard one say that he was better or happier after it ; several have said that they were worse : and many, having failed, have been made much worse.¹

The mental treatment which I have thus suggested will be in many cases sufficient. It will be more or less useful according to the degree of good sense possessed by the patient. A sensible man, who has been only ignorant on sexual subjects, who can understand evidence and is ready to believe those who are most likely to tell him what is true, will be cured when the truth is told. At the opposite extreme, the worst of the hypochondriacs will be almost incapable of cure : they will believe nothing hopeful ; they will be dull to all common-sense statements ; many of them will prefer to be guided by rogues rather than by honest men.

Between these extremes you will have various degrees of success ; and in the vast majority of cases time does good. Some few patients, whose hypochondriasis is a form of inherited insanity, become plainly insane ; some,

¹ Professor Humphry very justly points out that the functions of the sexual organs, and we may include with them the related parts of the nervous system, 'may be suspended for a long period, possibly for life ; and yet they may be sound and capable of being roused into activity.' The same can scarcely be said of any other parts. Holmes's *System of Surgery*, vol. v. p. 151.

though they marry and have duly regulated sexual intercourse, and may cease to have involuntary emissions, yet retain their other nervous symptoms, and continue hypochondriacal; but the vast majority get well. Some fall in love, marry, and are cured; some getting into the weighty responsibilities of life, have things to think about more important than their sexual organs, and in all, as they grow older, the spinal marrow becomes less irritable, so that the emissions, if they have been annoyed with them, become less frequent and are attended with less feeling of exhaustion.

Now, let me end by speaking, as I said I would, about the statements that miseries beyond any that I have told of are frequent consequences of the so-called functional sexual diseases. Epilepsy, all forms of paralysis, wasting palsies, amaurosis, impotence, insanity, idiotcy, emaciation, disease of the heart, phthisis, and whatever else may frighten the timid, or attract the morbid fear of the hypochondriac, are advertised by swindlers as the sure consequences of sexual disorders, unless they be averted by some secret treatment. And these men live on the insane and the foolish whom they can attract. They would do less harm, and be less encouraged in their frauds, if they could not refer to the works of some members of our own profession for opinions justifying what they pretend to be their own.

First among these false teachers is Lallemand. His picture of *Spermatorrhœa*, in its complete form, is a description of something which I believe to be unknown among Englishmen. It may be that there is no such disease in France: a wild imagination may have suggested it: but whether it can be found in France or not, I believe you will never see it here. I have not yet seen such a case as any of the worst cases which Lallemand

describes, nor any which would justify the general tone of his descriptions. To many others with as good opportunities as myself for seeing rare and severe cases they are as completely unknown: and I observe that English writers on the subject, when they wish to tell the worst things to which spermatorrhœa can be said to lead, speak, not from their own observations, but from what they believe to have been Lallemand's.

But setting aside his account, you will find, even among honest English writers, more serious troubles assigned to sexual disorders than I think they can justly be charged with.

In speaking of the symptoms of irritable spinal marrow associated with frequent seminal emissions, I indicated the fallacy of the argument on which it is held that these symptoms are the consequences of the emissions. The emissions, I said and (I hope) showed, are the consequences not the cause of the disorders of the nervous system; they may aggravate the condition from which themselves arise, but it is only in this sense that any measure of the disease can be ascribed to them.

Nearly the same may be said concerning the other supposed consequences of sexual disorder and sexual excess of whatever kind. Let us take, for instance, insanity; and what is said of it might be said of epilepsy and the other horrors assigned as consequences of spermatorrhœa and masturbation.

Masturbation and sexual excesses are commonly assigned as the cause of insanity in a considerable proportion of the insane inmates of asylums. But, I think you will find that no estimate is attempted of the probability that they who are said to have been thus made insane would have become insane without this or any other

excess. No doubt, in any man who inherits a disposition to insanity, excess of any kind, whether in sexual things, or in drinking, gambling, or any other, will hasten or determine the advent of insanity: but excesses do not make men insane who have naturally healthy brains. Think of the number of habitual drunkards whom we see dying here: the poor wretches are not mad, unless their drunkenness be a sign of it, an effect not a cause of insanity. Or, think of the number of sensualists in all classes of society, who, to the last degree and their latest life, stimulate and indulge their sexual desires in any way they can. These do not become insane in any larger proportion than do gamblers, or over-active politicians, or even the vehement students of science. To determine the influence of excesses in producing insanity you must count, not only the insane, but the sane who have committed excesses and retained their mental power.

And, even among the insane there are many of whom it would be truer to say that they masturbated because they were insane, than that they became insane because they masturbated. It is the same as with drunkenness. Habitual and paroxysmal drunkenness seem to me more frequently the consequence of insanity than insanity is of them. Certainly, the most marked cases are in those who are members of families in which insanity and other maladies of nervous centres are prevalent, and in those who are on other and previous grounds known to be not wholly sane or of average mind. Given a predisposition to insanity and, no doubt, any of these exciting or, as they may better be called, exhausting causes may induce it; may hasten it or determine its occurrence. And the greater the disposition, the less need be the exhaustion that will suffice: while in those in whom there is only the least, if any, disposition to insanity, nothing less than the

utterest exhaustion from excess may suffice to produce it, if even this may. The drunkards and sensualists who live and die sane are too many to let us speak of hard drink or masturbation, or sexual excesses as causes of insanity, unless under many reserves and conditions.

And what is true in respect of insanity is true in respect of other assigned consequences of sexual disorders. These disorders are effectual exciting causes of only such diseases as the patients are prone to ; and the proneness or predisposition is much nearer to the essence of the disease than is the exciting cause.

VI.

ON DISSECTION-WOUNDS.

In the 'Memoirs and Letters of Sir James Paget,' it is said by mistake that he gave this lecture to the students of St. Bartholomew's Hospital. He did not give it; but published it in the 'Lancet,' under the title 'Notes for a Clinical Lecture on Dissection-Poisons.' In a letter to the Editor of the 'Lancet' (June 3, 1872,) he says: 'While I was ill, I used to think that my own case might well be the subject of a clinical lecture, and the notes which I enclose might have served for such a lecture, if I had retained the surgeoncy of St. Bartholomew's. I beg you to use them in any way that you think best. They should be more complete, were it not that during acute illness one cannot record, and after recovery one cannot accurately remember, the signs and progress of a disease, or the thoughts which they suggest.' They were republished in 'Clinical Lectures and Essays.'

THE subject of dissection-wounds has, of late years, seemed less important than it was thought to be some forty years ago. When I was a student, it was believed that such wounds were not rarely fatal, and that frequently they led to severe disease. Now, they are often made light of. My recent illness disposes me to think this levity misplaced. It is very improbable that there should be any change in the virulence of poisons generated in the dead body; and if there be any change in the consequences of inoculation with them, I should think it due to the changes in the manner of treating them. In my student-days the first signs of inflammation following such wounds were generally treated with leeches, purgatives, spare diet, and other depressing means; now, as for many years past, the prescription is good food, wine, rest, and, above all, fresh air. You may be certain that

this is the better prescription; and I believe that if I could have availed myself of the whole of it, especially of the last two ingredients, I should have averted most of the troubles that I have lately suffered.

Let me now speak to you about these troubles. And, first, as to their source and the conditions on which they depend. The material with which I was infected was in the dead body of the patient on whose case I last lectured.¹ He died after lithotomy, with acute cellulitis at the back of the pelvis, and with acute pleurisy; both of which, I believe, were of pyæmial origin. The grounds for this belief I stated to you when, at the last lecture, I showed you the morbid structures removed after death. I refer to it now only that I may remind you of the probability that the inflammatory products of pyæmial disease are especially virulent after death. The most dangerous examinations appear to be those of women who have died with puerperal peritonitis; and most of the cases thus named are pyæmial. I can only suspect that the material which poisoned me was in the pleuritic fluid, in which my hands were long soaked; but what the poisonous material—the virus—was, neither this nor any record of similar cases enables me to tell.²

Whatever the virus was, it soaked through my skin; I had no wound or crack of any kind. Mr. Young, who began the examination, cut himself and suffered no harm. I had a sound skin, but one not impenetrable, and

¹ It is recorded in the *Lancet* of May 27, 1871, p. 711.

² A sad instance, in proof that it is not any ordinary decaying or decomposing animal matter which thus poisons, has lately occurred. One of the Museum-servants in the College of Surgeons, who had rarely passed a day during several years without frequent contact with animal bodies in all stages of decomposition, with macerating bones and preparations in spirit, took an appointment in the anatomical school of St. Thomas's Hospital. In a fortnight he wounded himself while preparing for dissection the body of a child dead of pyæmia. In a few days he died of septicæmia.

through it the virus worked its way. Mr. Young did what I advise you to do in any similar case. He washed his hands, sucked the cut part, made it bleed freely, and then took care of himself, and did not rub the cut part with nitrate of silver. He thus reduced his risk to less than that of the absorption of virus through sound skin.

This absorption is generally spoken of as if it were a strange and rare fact. Rare it may be, but strange it is not; for thus it is that, most commonly, the poison of chancre passes through skin and that of gonorrhœa through mucous membrane, and thus that the irritant matter of cantharides and other skin-irritants passes to the cutis, and from it may be absorbed. A wound or a crack that exposes a vascular surface is doubtless very favourable to infection by any virus; but it is not essential; I wish it had been and were still so.

Thus, then, this virus passed into me; and I will tell you presently some of the mischief that it did. But first let me say that this mischief would not have happened but that there was in me something that made my blood, or some of my textures, susceptible of such diseased processes as the virus could excite. For not all men can be made ill by a virus from a dead body, nor can the same man be made ill at all times; but there must be what is called a fitting soil for the virus to work in. We know no more what this soil is than we do what the virus is; we have to use figurative expressions; but we need not doubt that they imply facts, and that, for any living body to be made diseased by a dead one, there must be certain living materials which can be diverted by the dead ones from their normal relations and turned into a morbid course.

A chief interest in reference to these various susceptibilities of the influence of virus from dead bodies is, that one may become insusceptible. They who are day after

day engaged in dissections or in post-mortem examinations, usually acquire a complete immunity from the worse influences of the virus. They may suffer local troubles from it, and some among them may get that curious warty affection of the skin of the hands or fingers which Dr. Wilks described in the *Guy's Hospital Reports*¹; or they may lose health through the influence of bad air or over-work; but they do not suffer with any infection of the lymph or blood.

Such an immunity as this I enjoyed when I was demonstrator of morbid anatomy and made almost daily post-mortem examinations. It mattered not what was the disease of which the examined body died, or what was the state of my skin, sound, or cracked, or wounded; nothing hurt me; and this immunity lasted many years.

A similar immunity in the case of many fevers is possessed by those who have passed through one attack. It is, as you know, very rare to have a second attack of scarlet fever or of typhus; and it is, perhaps, more rare to have a second indurated chancre, or a second complete series of secondary symptoms. In these cases we believe that the first attack alters the blood or tissues in such a manner that they are no longer susceptible of the same morbid changes as they were, even though in all other respects they appear unaltered. But I think it is not in this way that the immunity from the infections of dead bodies is obtained; for though few demonstrators or others constantly engaged in morbid anatomy escape quite unscathed, yet some do so; and these may be enough to prove that the immunity is acquired by what we may call custom. Just as a man, beginning with small quantities of strong drink, and gradually increasing them, may never get drunk, even though he may at last

¹ Third Series, vol. viii. p. 263.

drink hugely too much ; or as a Styrian (if the stories be true) may take arsenic till he can hardly be poisoned with it ; so may anyone by custom become insusceptible of the evil effects of the corpse-poisons. It may be that his blood and tissues become less alterable by alien matters, or that the living parts acquire more power of assimilating or of excreting the dead materials that are introduced among them : how it comes to pass we cannot surely tell, but the fact of an acquired immunity seems certain.

I wish some of you would study these immunities more closely than anyone has yet done. They are of infinite interest in physiology, for they show a striking contrast between dead and living things. *Gutta cavat lapidem* is a pattern of many proverbs that express the popular knowledge that all dead things yield to the repeated application of small forces ; yet living things rather strengthen themselves against such forces. Not that this or any other contrast between dead and living things is absolute or constant ; yet this, like all the rest, is worth most careful study. And of still more interest in pathology are these acquired immunities ; for as yet we know scarcely more than the bare fact. Some immunities are local—such as that of which anyone will tell you who has had a long succession of blisters on the same place. He finds that at last he can be blistered there no more ; yet you may blister him on some other part. And such, I suspect, is the immunity from the virus of chancre which may be obtained through frequent inoculations ; but of this I have had no experimental knowledge. I think, however, that I have had personal knowledge of acquired immunity of another kind of inserted poisons—that, namely, of fleas, bugs, and the like pests of vacation-life. Let me commend this subject of study to any of

you who are not unwilling to be martyrs to science. I think you will find, as I have found in some Continental tours, that for the first night you may be driven half wild by the vermin of the bed or of the air, but that after a time you care less for them, and that at last you become indifferent to them; not because they leave you alone, but because their virus no longer irritates the blood or the textures that at first fiercely resented it.

A lady, on whose accuracy I can rely, has supplied this note of the case of a person having charge of her bees: 'He began by taking from four to eight stings every two days or so. At first his arms and hands swelled dreadfully, and he had great pains. Towards the end of a fortnight the pain and swelling lessened, and by the end of (he thinks) about two months of this treatment, not taking the stings so continuously as during the first three weeks, he had no suffering or even discomfort, though once about that very time twenty bees in a fury stung him all over the face. That was two summers ago, and he considers himself sting-proof for life. I have seen his hands covered with bees stinging him, and he has never paid the slightest attention to them. The sting will perhaps leave a slight red mark, if on the face, but there will be no swelling.'

This instance may seem a trivial one; yet I believe that in it you may find illustrations of much more serious things, even of that doctrine of syphilisation of which you have heard so much.

But now observe: this immunity, it seems, may be gradually lost, just as that after vaccination may be; the influence, as the expression is, gradually wears out. My case is evidence of this. Years ago no virus of a dead body could hurt me; but then came a time in which I made few or no examinations after death. I stood by

and watched others making them ; and I became again susceptible to poisons that were once innocuous. My blood and textures regained the state they had before ever virus was introduced into them, and I became again more poisonable.

Think how curious a fact this, which is only one of a large class, is. Being more susceptible of morbid influences, one seems less healthy ; but, in truth, one has become more healthy. Just as after vaccination, or after scarlet fever, one's blood or textures, or both, being so altered that the same poison will no longer act on them, seem the better for the change ; yet they are morbidly altered.¹ And then, for years afterwards, by the exact assimilation of the nutritive process, they are maintained in the same morbid state ; like a scar which, useful as it is, is yet a morbid structure. And as a scar, if not too deep, gradually wears out—that is, gradually reverts to the healthy skin-structure,—so is it with the blood and textures of the once infected person. Recovering their natural condition, they become again susceptible of infection ; becoming again healthy, they become what may seem weaker, and are more liable to disease.²

¹ Some opponents of vaccination have used this and a similar expression in my Lectures on Pathology as if they implied a mischief in vaccination. None but a silly or dishonest person could so use them ; but, with a very few strange exceptions, it is only by such persons as these that vaccination is opposed.

² The circumstances of an illness which Dr. Symes Thompson suffered many years ago, and which he has described in the *Lancet*, June 24, 1871, may illustrate this point. When he was Pathological Registrar at King's College Hospital he was constantly exposed to the various contagious fevers and, at post-mortem examinations, to the poisonous fluids of dead bodies ; yet, so long as he remained at his post, he escaped all infection, although his general health broke down from over-work. But on his return, strong and well, from a holiday in the pure air of the coast, he had so lost this immunity that he was immediately attacked with scarlet fever in a severe form, and also with erysipelatous inflammation of the hand and arm, and mischief in the axillary lymph-glands, following a scratch on his finger at a post-mortem examination.

Think, again, of the long time during which facts like these prove that a process of recovery from disease may continue before it is perfect. The years through which a man must pass before he becomes liable to a second attack of these diseases tell the time that is required for his complete recovery from the first. Let the fact teach you both patience and hope in your treatment of the consequences of disease. It makes me believe that I might now, with perfect safety, examine any dead body whatever.

Now let me tell you, with commentaries, what the virus did in me. The examination was made on February 4, and after it I finished a long day's work, feeling unharmed. On the 5th, which was a Sunday, I felt not ill, but tired, and I spent the greater part of the day idly, falling asleep over good books. On the 6th I lectured, in the morning, on the morbid structures obtained from the examination, and the theatre was, as usual on Mondays in the winter, very cold. I was chilled and very tired; but a heavy day's work had to be done, and I did it. I had observed three or four small pustules on my hands, especially one on the back of my left hand, but they caused no discomfort, and I had no suspicion of being hurt till about five o'clock, when I felt my left axillary glands tender, and could not press my arm against my side. At half-past eight, when I got home, I was cold and ill; the mischief had begun.

I mention these things that I may illustrate, as I have often before done, the influence of fatigue in developing disease, or in at least making one susceptible of it. I can be as sure as of anything which has not occurred, that if I could have rested for two or three days after the insertion of the virus, it would have done me little or no harm. I cannot tell you whether it is by mere diminution of a

normal power of resisting changes, or (as Dr. Carpenter has shown to be more probable) by the production in the fatigued organs of some material on which no bad poisons may multiply or flourish; but you will find in every day's practice that fatigue has a larger share in the promotion or permission of disease than any other single casual condition you can name.

Thus, then, I was prepared for receiving injury, and the injury was supplied in some material of this dead body. And I repeat that this material was probably something special enough to be called a virus or poison; for although any decomposing organic matter may in some persons give rise to the worst forms of blood-poisoning, yet I have no reason to think it would have done so in me. Not a day had passed for many months without my hands being in contact with pus and other decaying or decomposing organic matters; yet none of these poisoned me, though I was often as much fatigued as when I fell ill.

I wish that I could tell exactly all the signs of illness that I so anxiously watched; but during acute disease one cannot record, and after recovery one cannot well remember the daily progress of a case. I can only tell the general consequences of this poisoning.

The first thing observed was a few small pustules on the hands, very trivial-looking things, which appeared on the day after the examination, and in the next week or ten days dried without discharging or causing any local trouble. I think they were only local effects of the simply irritant fluids of the body, or of the carbolic acid oil, with which I had uselessly though thoroughly rubbed my hands before beginning my part of the examination. I see no reason for supposing that the material which poisoned me was from any of these pustules.

The first sign of the general poisoning was (as I have said) the pain in the axillary lymph-glands. No lymph-vessels could be seen or felt up the arm at any time; the absorbed material traversed them, but did not irritate them; but the glands enlarged, and became painful to a degree far surpassing the swelling or any other sign of inflammation in them. I do not know whether this exceeding painfulness was due to something in the poison, or was dependent on some peculiarity in my nervous system. I have seen it in one other case of poisoned wound, in which, as in mine, it indicated no great severity of inflammation in the glands. With me it slowly diminished, but did not quite cease till I was nearly well again, although no considerable morbid changes took place in the glands. They were large and painful; nothing else.

Next after this affection of the axillary lymph-glands came widespread inflammation of cellular tissue. But before saying more of this I ought to tell what may have had some influence in determining the course of my case—namely, that my axillary glands were already damaged, and may have been less penetrable by fluid than they should be. More than thirty years ago, when I was a student, I had an irritable large pustule on a finger, due, I think, to some irritation in dissection, and this was slowly followed by suppuration of my left axillary glands. After discharging for many weeks, it healed with a deep scar and shrivelling and partial calcification. Thus damaged they may have too much hindered the course of the absorbed fluid; but I doubt their doing so, for I have never been conscious of any obstruction in them; and in other cases of patients with healthy lymph-glands the same inflammation of cellular tissue as I had occurs.

This inflammation, which became evident four or five days after the infection, extended quickly from the axilla

up the left side of the neck, over and below the clavicle, and down the back nearly as far as the ilium. Observe the range within which the inflammation was limited, for in this, as in other cases, it was in the range of lymphatics directly, or not far from directly, connected with the lymph-glands first affected. A frequent site for such cellular inflammation is down the side of the chest, or over or beneath the pectoral muscles. Thus it was with Mr. Bloxam, who was poisoned about a fortnight after me, and in whom acute pleurisy with effusion occurred on the same side. But, so far as I know, cases do not occur of similar inflammation on the opposite side, or separated far from the lymph-glands corresponding with the poisoned part, unless it be at a later period of illness, when pyæmia is established. This seems to be a point of distinction between these dissection-poisonings and pyæmia; their effects are at first, however severe, comparatively limited to the part poisoned, and to the lymphatic vessels and glands, or to the cellular tissue, nearly in relation with it. They may lead to pyæmia, but they do this only by secondary changes, or, as it were, by some accident.

The range of cellular inflammations in these cases, following, as they do, on the affection of the axillary glands, seems to indicate that they are due to arrest of lymph in the affected parts, and to its being poisoned by reflux from the glands. Thus poisoned it would at once infect the cellular tissue in contact or close proximity, and hence would be derived the spreading inflammation, much like phlegmonous erysipelas, with sloughing or diffuse suppuration.

In my case the inflammation, at first widely diffused, gradually concentrated its effects in two places—first at the back, nearly over the angle of the sixth rib, where

suppuration was evident about a fortnight after the first appearance of the swelling, and, about a week later, under the edge of the trapezius, just above the level of the clavicle. Both these abscesses were freely opened. The first was seated in the deepest part of the subcutaneous cellular tissue, and the second under the cervical fascia. The first suppurated very freely; the second scantily. Both healed soundly in five or six weeks from the time of opening them.

The position of this second abscess deserves notice, for it was probably due in part to the old damage of the axillary glands, of which I have already spoken. It was such an abscess as might have pointed in the axilla, and I always felt as if it would do so, for hardness and pain could always be detected there. But it seemed as if the axillary tissues would not yield, and therefore the abscess extended upwards, above the apex of the axilla, to the subfascial tissue beneath the edge of the trapezius. Both these abscesses were opened early—that is, so soon as fluid was clearly discovered in them. And I felt the comfort and utility of this practice; for though they had given me very little pain, and had been soothed with poultices, yet they seemed to keep alive my fever, and especially before the pointing of the second I had chills and exhaustion, which were evidently remedied by its being opened. In the opening I enjoyed the safety which I have often conferred on others by the adoption of Mr. Hilton's plan. The abscess lay very deep, and was very small, under the edge of the trapezius, and with integuments and cellular tissue so thickened over it that all the landmarks for incisions were lost, and the district was one in the depths of which a knife, however skilfully used, might have given me serious trouble. The director penetrated the abscess safely, and the forceps sufficiently

dilated the opening, and I had self-evidence on which to urge you to use Mr. Hilton's plan in all operations on abscesses in dangerous regions.

After the opening of the abscesses the infiltration of the cellular tissue about them cleared up slowly, very slowly; and, more than once, patches of thick œdema appeared over the left ilium as if suppuration would take place there; and when erysipelas came on there was much more than usual œdema with it, as if there were still some hindrance to the free movement of the lymph.

The general illness that attended these suppurations was not severe. When it was evident that abscess would form—that is, a fortnight after infection,—I was sent to Norwood, and with the fresh air and quiet of the place, I gained strength, and could eat and drink well, and digest very well, and seemed floating into convalescence. But just before the evident suppuration of the second abscess, I had chills every day, and after them heat; and with these great loss of power and general distress—distress so keen that it seems strange that I should now be unable to describe it, or even clearly to remember it.

This general illness, this constitutional disturbance, was, I believe, the beginning of erysipelas. But before speaking of this, let me tell that the pus from my abscess appeared to have more irritant properties than ordinary pus. For my children's nurse, who made and changed my poultices, pricked her finger; and this was followed by very acute inflammation and suppuration, extending from the puncture over all the hand and forearm. The same thing happened, through a similar accident, to Mr. Bloxam's nurse, one who was habituated to pus of all ordinary kinds, and had not suffered from it.

Of the erysipelas that affected me after these abscesses,

beginning about a month from the time of infection, I need not say much, for it had no remarkable feature, unless it were in the degree of subcutaneous œdema which predominated over inflammation of the skin, and remained very long. Beginning near the wound in the neck, the erysipelas spread slowly over the chest and back, down the left arm, and over parts of the thighs. The eruption was much more extensive on the left side than on the right; it slowly cleared up, and after desquamation no trace of it remained, unless it were in a weakness of the minute blood-vessels of my arm; for after my bath, for many weeks, the affected part of this arm appeared dusky and mottled.¹

It was during the erysipelas that my general health suffered most: but my recollection is not clear about anything but the feelings of intolerable restlessness, which nothing but wine or morphia would tranquillise, and of the interest with which for many days I watched the progress of my own case, fancying myself an intelligent observer. At last, after the erysipelas had been extending for about ten days, and at the end of nearly six weeks from the infection, there came what seemed to me like a crisis. During the night in which my pulse and temperature were at their highest I had a profuse sweating and a profuse flow of urine, such as I never had in my life before; and next day my pulse and temperature had come down to what may be deemed safety-points, and I was conscious of returning health.

In the treatment that I received during the erysipelas, I am sure that quinine was very useful. I generally took three or four grains of the hydrochlorate three times

¹ Even now, after eight years, a cold bath makes the left arm look more dusky than the right, though I am not conscious of any other difference between them. (*Note to 2nd edition of Clinical Lectures, 1879.*)

a day, and it always (I think) lowered my pulse, and diminished my restlessness and (I believe) my temperature. And I wish I could tell the comfort that morphia gave me—whether in bringing sleep, or in changing the unrest that always increased towards night into a happy and complacent wakefulness almost as refreshing as sleep. It was well that pleasure unnaturally obtained should have a penalty; and this was in the dryness of mouth, which seemed due to a total suspension of the secretion of saliva, and which became at last even less tolerable than restlessness. Locally, collodion, freely applied as soon as an erysipelalous redness appeared, gave great comfort. It did not hinder the spreading of the erysipelas; but it relieved the itching and heat of the eruption, and it prevented the horrid itching of the desquamation, which was intense, and was renewed for many days on exposure to air, at every place to which the collodion was not applied. These things were certainly beneficial; but I suppose that those which most helped me to a safe passage through the illness were, a judicious moderate use of food and wine, and very wise and gentle nursing.

This erysipelas, it may be believed, was part of the effects of the poison of the dead body—an issue of the blood-poisoning. Not such, I think, was a pneumonia with which I suffered twice, and which added greatly to the risk and length of my illness. This was, probably, personal; due only indirectly, if at all, to the poison—due rather to the susceptibility of my lungs to the inflammatory process. For I had had acute pneumonia five times during the eighteen years before this illness. All these attacks occurred after severe over-work, with deficient food and exposure to cold; and the manner in which they have cleared off, leaving my lungs unimpaired in structure, has made it nearly certain that they were rheumatic or gouty.

But, however this may be, the pneumonia, of which one attack commenced only two days after the infection, and the other a week after the disappearance of the erysipelas, must be ascribed to me rather than to the poison. They passed through their usual course, and left my lungs sound again ; but I ask your attention to them as an illustration of one of the ways in which a specific disease may be complicated or modified by the personal constitution of the patient. Here was an instance of what one may call a specific poisoning ; and one of the first things following it was pneumonia. Associated as this was with evidence of poison in the lymph-glands, it might have been thought pyæmial, or in some way due to the specific poison in the blood. Yet it was only such a pneumonia as I might have had without having been poisoned, or such as might occur in me in any feverish illness from whatever source.

Keep such facts as this in mind. They show that there is no disease so specific but that its signs may be confused or complicated with the things that are peculiar to the patient. Syphilis is a specific disease as sharply defined as any, but its course and appearance in a scrofulous man and in a gouty one are very different. Vaccination produces a well-marked specific disease ; but in one patient it may be followed by inflammation of lymphatics, in another by eczema, in others by various other troubles : but all these are due in only a minor degree to the vaccination ; they come out from the personal constitutions of the several patients which are disturbed by the vaccination, as they might have been by anything else producing some slight fever.

This is not a mere question of doctrinal pathology. It is among the first necessities for success in practice that, in the several phenomena of a disease observed in any

patient, you should be able to estimate what belongs to the disease and what to the man. A farmer may as well expect success if he sows his fields without regard to their soils or to the weeds that may 'of themselves' come up in them, as one of us may expect it if we treat diseases without exactly studying the constitutions of those in whom they occur.

Thus I have given a sketch of my three months' illness, and some of the thoughts which it suggested to me. But I ought to say that my case showed only one of many forms of disease that may be produced by the poisons of dead bodies. The suppuration of lymph-glands, which I had many years ago, is another. But besides such as these, you may find cases of trivial local inflammation; of direct and simple erysipelas; of spreading, suppurating, or sloughing inflammation of the cellular tissue of the hand and arm; of pyæmia; and of the fiercest septicæmia. And it is remarkable that different effects may be produced by the same poison acting on different persons. Mr. Erichsen mentions a case in which six students were infected by the same body; 'two had suppuration of the areolar tissue under the pectoral muscles and in the axilla; one was seized with a kind of maniacal delirium; a fourth had typhoid fever; and the other two were seriously though not dangerously indisposed.' I advise you to read-up the subject in his *Art and Science of Surgery*.¹ He has given an excellent account of it; and so has Billroth in his and v. Pitha's *Handbuch der Chirurgie*.²

Sir William Lawrence used to say that he had not known anyone recover on whose case more than seven had consulted. Our art has improved. I had the happiness

¹ Vol. i. p. 151, 5th edit. 1869.

² Bd. i. Abth. ii. Heft ii. p. 79; Erlangen, 1867.

of being attended by ten; Sir Thomas Watson, Sir George Burrows, Sir William Jenner, Sir William Gull, Dr. Andrew, Dr. Gee, Mr. Cæsar Hawkins, Mr. Savory, Mr. Thomas Smith, and Mr. Karkeek. In this multitude of counsellors was safety. The gratitude I owe to them is more than I can tell—more than all the evidences of my esteem can ever prove.

VII.

NERVOUS MIMICRY.

*Clinical Lectures, published in the 'Lancet,' 1873, and republished in
'Clinical Lectures and Essays.'*

LECTURE I.

A GROUP of cases of great practical importance is distinguished by this fact: that a nervous disorder produces an imitation or mimicry of organic local disease. In some of these cases the mimicry occurs without any substantial disease whatever; in others it gives features of extreme severity to a disease which, in a normal condition of the nervous system, would be trivial or unfelt.

Much has been written on this subject—much that is good; but, if I may judge from the difficulties still often found in discriminating between the real and the mimic in diseases, there is need of writing more.

Cases of this kind are commonly included under the name Hysteria; but in many of them none of the distinctive signs of hysteria are ever observed, and from all of them it is desirable that this name should be abolished. For it is absurdly derived and, being often used as a term of reproach, is worse than absurd. To call a patient hysterical is taken by many people as meaning that she is silly, or shamming, or could get well if she pleased; and no doubt there are patients of whom some of these things may fairly be said; but in many more, hysteria,

especially in the form of an unwilling imitation of organic disease, is a serious affection, making life useless and unhappy and not rarely shortening it.

At any rate, let us, if we can, discard the name of hysteria from surgery. If it is to be retained at all, it may be for the cases of patients with hysterical convulsions and sense of suffocation, with the abdominal flatulence, the nervous urinary secretion, and those other associated signs of nervous disorder which are not imitations of other diseases, and are not themselves imitated. These are characteristic enough to deserve a distinct name, and hysteria will serve, at least as well as hypochondriasis and melancholy do for what they signify. But the characters of nervous mimicry are also distinct enough to make a separate group with another name. In English we may speak of nervous mimicry; in untranslated Greek, of neuromimesis. To patients and their friends the maladies may be said to be due to extreme nervous sensibility; or, if they also prefer Greek, we may call them hyperæsthetic or hyperneurotic;—anything but hysterical.¹

The chief practical interest of these cases is in the diagnosis between them and the organic diseases imitated in them; and as it is only in clinical study, and on clinical grounds, that they can be grouped, so I shall keep to a clinical view of them, and shall speak of their pathology only as subservient to their diagnosis. I am the more content to do this because, in Dr. Russell Reynolds's essay on Hysteria in his *System of Medicine* and in Dr. Anstie's lectures on it in the *Lancet*, you may easily find the whole subject recently and accurately taught. Indeed, if it were not that I may treat the surgical part of the

¹ Hysteria may be fitly called *Mimosa* from its counterfeiting so many diseases, even death itself. Coleridge, *Table-Talk*, 1852, p. 81.

subject more fully than they, I should not have seen room or occasion to lecture on it at all.

Now, there is scarcely a local organic disease of invisible structures which may not be mimicked by nervous disorder. You hear of hysteric cough and hysteric aphonia, of hysteric dyspepsia and paralysis, of hysteric joints and spines; and there is scarcely any of these disorders in which the mimicry of real diseases is not, sometimes, so close as to make the diagnosis very difficult.

The means for diagnosis are to be sought—(1) in what may be regarded as the predisposition—the general condition of the nervous system on which, as on a predisposing constitution, the nervous mimicry of disease is founded; (2) in the events by which, as by exciting causes, the mimicry may be evoked or localised; (3) in the local symptoms of each case. And I shall speak of these things in this order; though it is the wrong order for the actual study of a case. In every clinical study, the examination of the present disease should precede that of its probable causes; though for lectures and reading the reverse order is usually the better.

First, then, as to the general condition of the nervous system which disposes to mimicry of local disease. In all well-marked instances there is some prominence and apparent excess of nervous action, leading to the general expression of the patient's being nervous or of nervous constitution. Neuromimesis cannot be found in all persons alike, or in any person at all times. It may be regarded as a localised manifestation of a certain constitution; localised, that is, in the same meaning as we have when we speak of the local manifestation of gout or of syphilis, or of any other morbid constitution which we regard as something general or diffused, though distinct

witness of it may be in only one or more parts. And the nervous constitution, like others, is inherited in different degrees of completeness or intensity; and may, like others, become less or more complete or intense according to the conditions in which it has to live.

As to what is, verily, the peculiarity of the nervous constitution, I believe we have nothing fit to be called knowledge. It is even hard to give fit names to what we may suppose it to be. We may speak of the nervous centres as being too alert, or too highly charged with nerve-force; too swift in mutual influence; or too delicately adjusted, or defectively balanced. But expressions such as these, or others that I see used, may be misleading. It is better for us to study the nervous constitution in clinical facts; and the life of almost any patient with very marked nervous mimicry will supply materials for the study. I have, indeed, seen a few in whom I could find no other nervous error than the present mimicry of some disease. But in the great majority there is either history or present evidence of a characteristic nervous constitution, such as may serve towards diagnosis. Some have been, or are even now, truly hysterical; subject to fits of irrepressible laughing, crying, or sobbing, or to convulsions of various hysteric kinds. But you will find nervous mimicry in very many who have never been hysterical. In some the sensibility is always too keen, whether for pain or pleasure. In these the pain of an injury is much more severe than what we may suppose to be the proper average of pain producible by such an injury: it lasts longer; outliving all the other consequences of the injury. And, as to pleasure, as a patient said to me, who suffered what she called tortures from ordinary sources of moderate pain, 'the pleasure of music is an agony.' But not all have this compensation

of feeling pleasure as keen as pain : for many are habitually neuralgic ; they suffer with headaches, dartings in limbs, still more often with spine-aches and the like, and are, as one may say, very painful persons,—altogether hyperneurotic in their relations to pain, but not to pleasure. In some of these patients, again, the mutual influences of the mind and body are too active. If they have ever fainted or vomited, for instance, the recollection of the circumstances in which they did so makes them faint or sick again. In some, all impressions are too widely and vehemently reflected ; the irritant that healthy people bear unobserved produces convulsions, or some other seeming serious disturbance. An intestinal worm, for instance, which some patients would not feel, may in these produce the signs of all manner of diseases. And some have already had mimicries of various other organic diseases, and will tell of all the sensation-signs of these diseases, without having a single mark of any organic change produced by them.

One of the most frequent conditions in those in whom the nervous mimicries occur is a singular readiness to be painfully fatigued by slight exertion. This is most marked in those with spinal neuromimesis, but may be found in many more ; and in some is the most marked sign of the erroneous state of the nervous system. To most of us the sense of fatigue produced by even excessive exercise is scarcely distressing : only a great excess, tending to exhaustion, would be felt as really painful ; and the worst is soon relieved by rest. But these nervous patients become utterly fatigued in even slight exercise, and their limbs and their backs, though they may look muscular and strong, ache horribly and very long ; so that, not rarely, the attempt at more than usual exercise is followed by great suffering, by sleepless nights, and sometimes by

nausea and vomiting. Their sensations are like those of the painful fatigue which convalescents from acute disease feel after doing too much ; but I believe there is never attendant fever such as convalescents in that case usually have ; the likeness is only in sensation."

Some or all of these, and of other forms of defect, or error, or turbulence—call it what you will—of the nervous system, you will nearly always find or hear-of in patients with nervous mimics ; and in any study of a case these conditions or their absence may add to the grounds for diagnosis. But it is, I think, a fact of singular interest that, in even the most turbulent of these nervous systems, the disturbance very rarely takes the form in which morbid nervous influence produces, not mimic, but real organic changes. Of the things imitated hardly one is ever realised. I have not yet seen in any hysterical or neuromimetic patient an instance of herpes zona ; not even in those who have suffered long with that infra-mammary pain which is not far unlike the neuralgia commonly preceding the eruption. I have not seen an instance of rheumatoid stiffening of joints, like that which sometimes ensues in disease of the spinal marrow, even in the most tedious of neurotic joints ; nor one of the glossy fingers, or the eczema, or the centripetal wastings, or, in short, any of the organic diseases of peripheral parts which are associated with injuries or organic diseases of nerves or nervous centres.¹ Neither are these patients especially liable to any forms of fever or of blood-poisoning ; they incur as little risk from operations as any class of healthy persons. The fact—I think it is one—is important in both diagnosis and pathology ; and it is not falsified, I

¹ I have an impression that yellow and other pigment-marks at the sides of the forehead, under the eyes, and on the abdomen are significant of a neurotic constitution.

believe, in any but a few ambiguous cases in which what seems a mimicry of disease of the spinal marrow passes into real disease, and leads to extreme wasting of the lower limbs and arrest of the growth of nails. The contrast is the more notable when we consider that the distribution of blood is, in many of the mimic cases, greatly affected. Heat and cold of the same part rapidly succeeding one another, flushing and pallor, turgidity and collapse,—all these are frequent, striking, and capricious in the nervous mimicries; but, after months and years of their occurrence, not one organic change may be discerned. You may find in our Hospital Reports¹ the case of a gentleman who consulted me because, for several years, whenever he walked far or fast, his feet became cold, white, and numb—‘dead,’ as they are called; and then, when he rested, they flushed red and hot and were turgid with blood, distending even the veins of the leg. Yet, after years of such disturbance, all the structures of his feet were as healthy as any of yours are. And I urge you to study carefully such cases. They are rare, but you are likely to meet with them, and when you watch them think of what may happen if such changes as you see in the skin, now looking bloodless and now over-filled with blood, can, on as little provocation, happen in the brain or spinal marrow, in the heart or in an ovary. What startling and yet what harmless disturbances they might produce! what mimicries of grave disease!

But now, to add again to the grounds of diagnosis: it is seldom that patients with well-marked nervous mimicries have ordinary minds—such minds as we may think average, level, and evenly balanced. You may, indeed, find among them some commonplace people, with dull, low-level minds; but, in the majority, there is something

¹ Vol. viii. p. 67.

notable, good or bad, higher or lower than the average—something outstanding or sunken. This something is, in different cases, so various that it is impossible to classify or even to enumerate the diversities. But be clear that these patients are not all silly, or fraudulent. Nothing can be more mischievous than a belief that mimicry of organic disease is to be found only or chiefly in the silly, selfish girls among whom it is commonly supposed that hysteria is rife or an almost natural state. It would be safer for you to believe that you are likely to meet with it among the very good, the very wise, and the most accomplished women. But it will be safest if you believe only that, in any case of doubt whether a local disease be organic or nervous, it adds something to the probability of its being nervous if the patient has a very unusual mental character, especially if it be unusual in the predominance of its emotional part; so that under emotion, or with distracted attention, many things can be done or borne which, in the quieter mental state, are felt as if impossible or intolerable. And this probability of mimic rather than real disease will be much increased if the symptoms seemed to follow any great or prolonged mental tension, or if the patient's mind be set, in much more than the ordinary degree, upon the real or supposed disease. In all the well-marked cases of nervous mimicry, and in the less marked in only a less degree, the malady determines the general current of thought, and often of the whole life. Egotism has its keenest life at and about the supposed seat of disease. If the malady be not always uppermost in the thoughts, it seems always in an under-current, rising at every interval between the distractions of work or play.

This influence of the mind on the pain and other characters of mimic disease can hardly be over-estimated.

It is very erroneous when the patients are charged with imagining or inventing their sufferings. Their only fault, if it may be so called, is that they attend to them too much. Attention makes all pain sharper; without it there would be little or none; and attention long exercised becomes keener, more direct and definite; and so the perception of pain becomes more intense. And, then, if the attention be fatigued or averted, it is constantly apt to be recalled by mental associations. These, very commonly, are the causes of relapses of nervous mimicries. And so are anxieties and fears; as in the common cases of young persons whose mimicries are renewed or become emphatic in the anticipation of marriage.

The contrast of the mental states of those who have real and those who have imitated local diseases is often very striking and of great help in diagnosis. Few patients with real hip-disease or real spinal disease, for instance, think half so much about their ailments as they do whose nervous systems imitate those diseases. In this egotism they resemble hypochondriacs; yet commonly with a great mental difference, in that those with neuromimesis are not distressed with constant forebodings of greater mischief; they do not suspect that everything they feel is a token of something much worse than can be felt; rather, they are content and often almost happy in their afflictions. While the hypochondriacs are in a panic on account of some trivial aching, the neuromimetics will talk of their agonies with calm or smiling faces, or with half-closed quivering eyelids; some seem proud in the immensity of their ailments; in some, there seems an unbounded capacity for the enjoyment of suffering.

This egotism in relation to the imitated diseases gives to many patients an appearance of great wilfulness. Some, indeed, are very strong-willed; some are so for all

the good designs in which they engage, and some with a thorough, almost diagnostic self-service. But strong will is, I think, less common among these patients than is a want of will. Sometimes there is a general feebleness of will: the patients can do nothing for themselves; can trust themselves in nothing; but commit themselves to some one with a stronger will and an appearance, if not a reality, of more knowledge. Hence, among these patients are the most numerous subjects of mesmerism, spiritualism, and the other supposed forces of which the chief evidence is the power of a strong will over a weak one. But more often you will find a feebleness or complete negation of will in reference to the supposed seat of disease, while towards other things the will is strong enough. You may find the strangest inconsistencies in this respect. A man who has intellect and will enough to manage a great business, or to travel with much inconvenience and write clever books, cannot will to endure sitting upright for ten minutes, or cannot distract his attention enough to be indifferent to an unmeaning ache in his back. A girl who has will enough in other things to rule the house has yet not will enough in regard to her limbs to walk a step with them, though they are as muscular as ever in her life. She says, as all such patients do, 'I cannot'; it looks like 'I will not'; but it is 'I cannot will.'

I think it is to this same weakness of will that we may attribute other things often observed in the worst cases of neuromimesis, especially the disposition of the patients to imitate or assume symptoms of disease that they have seen or heard of, such as the deformities of diseased joints, the lameness or paralysis associated with spine-disease, and the supposed distinctive pains of cancer. No doubt there is sometimes intentional fraud and lying in these cases; but in many more I think you may be sure that patients

do not study the imitation or deliberately determine to practise it. Rather they are, in respect of will, like children, who almost involuntarily imitate diseases; for instance in stammering, limping, and so on. I think that many persons, even such as have good nervous systems, must be conscious that it requires effort—that is, a full exercise of will—to avoid these imitations, and to disbelieve or disregard sensations imitative of those endured by others. And in the frauds which some of these patients practise I am nearly sure that the fault is rather in weakness of the will than in its perverse strength. As other people cannot resist thieving or drinking, so these cannot resist, have not will enough to resist, the inducement to fraudulently exaggerate their symptoms, or even to invent some. It is often very hard to distinguish between the frauds of the wilful and those of the will-less; but I have seen no reason to believe that wilful fraud in disease is much more common among those with nervous mimicry or hysteria than it is among others.

If you study nervous mimicry in all the varieties of mental strangeness that may be associated with it, it may often seem to you an entirely mental disorder, due only to imagination, or to intense attention directed on one place, or to adoption of signs heard of, or, in many cases, to an insanity of the intellect; and it is not easy to find sufficient evidence that it is not so. Imagination, fear, and keen attention, association, and the tendency to imitate distresses seen or heard of, might produce all the morbid sensations of which patients tell, and might give form and intensity to pains from any casual source of real disease; and as for the postures observed, for instance, in mimicries of diseased joints or spines, these, it may be said, are only such as are assumed instinctively for the relief of pain; and in an intensely sensitive person, with

no substantial reason for more than a slight pain, it is probable that the subjective intensity of pain would bring about the same postures of relief. Similarly, flushings and transient heats might be all of mental origin, as blushing is. If you study these mimicries from the mental side, you may, I say, easily find reasons for thinking them mere mental errors—insanities—rather than erroneous workings of sensitive and motor nerve-centres; and you will be almost convinced of this view of the case if you will read of the multiform and profound influences of the mind over the body in Dr. Tuke's recent book¹ on this subject, or in any similar work. But I think I may assure you, that to regard all mimicries of organic diseases as essentially mental errors would be bad pathology and worse practice. Let me put the case before you very briefly, for I am lecturing on diagnosis, not on pathology.

Some mimicries are essentially mental; such, for instance, as those in which patients, out of mere fear and keen attention, acquire the pains of cancer, and localise them in healthy parts; and in nearly all mimicries a mental influence may be discerned, just as it may in nearly all real diseases in which consciousness remains—an influence often impossible to separate or weigh, generally increasing with the duration of the disease, yet not essential to it whether it be real or mimic. But in some mimicries it is hard to discern any mental influence at all. Some are imitations of diseases very far from mental associations—in the cases, for example, of intestinal distension, constipation of many days' duration, constant vomiting and aepsia, rapid heart-action with slow breathing, largely pulsating arteries, and phantom-tumours. Some are found in commonplace, ignorant, and

¹ Tuke, *Influence of the Mind upon the Body*, 1872.

slow-minded people who never saw or heard of the diseases imitated in them. Some occur in children who could neither imagine nor act what they tell and show, though as they grow-up they may become those in whose successive mimicries the mental influence takes a constantly increasing part. And, to end whatever may be ascribed to mental influence, it can produce mimicry of organic disease in only certain persons whose nervous organs seem wholly prone to this manner of disorder, and whose spinal and ganglionic systems must be deemed erroneous, as well as, or more than, their brains. For nervous mimicry is not very frequent among the evidently insane, and among the sane there are many who cannot bring about a mimicry of disease by any effort of imagination or direction of the mind. Among these I am happy to count myself. I have tried many times, carefully, and with good opportunities, but have always failed.

LECTURE II.

In the last lecture I spoke generally of the characters of the nervous constitution commonly observed in those who suffer with the mimicries of organic diseases. You may believe, that, however much the circumstances of a life may favour the development of such a constitution, it would not be well-marked unless it were inherited. And facts relating to inheritance deserve great weight in the diagnosis of any doubtful case of nervous mimicry.

In looking for indications of this inheritance you may not find that, in the same family, there are or have been many cases of similar mimicry of disease; but it is a fact of not less weight if, in the same family, various other

forms of nervous disorders—especially of such as are, for convenience, called functional disorders—have been observed. Thus among the relatives of those with neuro-mimesis it is common to find cases of mental insanity, extreme ‘nervousness’ and eccentricity, stuttering, convulsive and emotional hysteria, various neuralgiæ, extremes of mental character whether good or bad, and sometimes (but I think less frequently) epilepsy and paraplegia. These evidences of family-relations may help in diagnosis, just as, in the diagnosis of a doubtful tuberculous disease, it is important if, among the members of the patient’s family, there have been many more than an ordinary number of cases of pulmonary tuberculosis, of tuberculous lymph-glands, of lupus, or of perforating ulcers. So, in the diagnosis of a doubtful case of gouty disease, it is important if relatives have had either typical gout, or many of the less marked forms of gouty affections of the skin, or kidneys, or any other part. It is a general fact, that if any constitutional disease is prevalent among many members of the same family, it affects them variously, in different degrees of intensity, and in different parts and textures; but by this diversity the total value of the evidence of inheritance, as bearing on a doubtful case, is not diminished.

The family-relationship between the nervous mimicries and mental insanity is specially worth considering. It helped us to the diagnosis, as some of you may remember, in the cases of two girls who were at the same time in Sitwell ward; one with strongly marked mimic disease of the hip; the other with very slightly marked real disease. The mother of the former was in a lunatic asylum; the mother of the latter died tuberculous.

I believe that a large majority of the worst cases of nervous mimicry occur in members of families in which

mental insanity has been frequent. And the fact is important, not only for diagnosis, but for pathology. It may serve to strengthen the view that nervous mimicry is a mental disorder; but I believe it may be more rightly read as an indication that, whatever mental insanity may be as a disorder of some portion of the brain, the like is nervous mimicry as a disorder of other nervous centres. Surely, any nervous centre may 'go mad' as well as any part of the brain which is appropriate to any part of the mind; and may be mad in different ways, imitating the idiotic, the emotional, the maniacal, or other forms of insanity. But I only suggest this for your study, and by the way. Let us seek for other grounds of diagnosis.

Nervous mimicry is much more frequent in females than in males; more frequent from the beginning of puberty to middle age than in either earlier or later life; more frequent in the more cultivated than in the rougher classes of society. But in what proportions it occurs in each of these several groups it seems impossible to tell with any fair approach to accuracy; for no one has a field of observation so general, or so equally inclusive of all these groups of persons, as to be able to count fairly among them. I should disbelieve any statistics pretending to settle the proportions accurately. You may be sure that neuromimesis is most frequent in young women of the more cultivated classes; but you may be equally sure that it is not so rare among men, or children, or at any age, or in any social condition, as to make it unreasonable to suspect it in any case of obscure disease. You had better not let any such case pass without asking yourself, Is this disease, or any part of it, mimicry? Some of the worst cases of mimicry of disease of the spine and pelvis that I have seen have been in men and women of

mature age ; some of the worst of joints in young children ; some of the worst of all kinds in poor people.¹

It is equally or more difficult to find any general characters of health, except those of the nervous system, which may help to the diagnosis of mimicry from real disease. Some of the patients are in various degrees gouty ; some are tuberculous ; some scrofulous. The nervous constitution may be mingled in various proportions with any others, adding its own characters to theirs, making pain more intense, spasm more severe and frequent, or receiving from other constitutions characters of organic disease very difficult to eliminate.

These various combinations of constitutions must be well studied. The most troublesome is the combination of the nervous with the gouty constitution. For in one in whom gout is not complete there are never wanting strange sensations—of tinglings, burnings, pains, pressures. In the neuromimete these become intense ; his nervous system defines them or gives them form ; and the difficulty of diagnosis becomes extreme. And as the combination with gout is the most troublesome, so that with tuberculosis is the most dangerous. It is through this that many die whose maladies have been made light of, and called, with a sneer, hysterical, or only nervous, or the like. They die, and often die young, not through their nervous malady, or anything directly affected by it, but through tuberculosis or some allied disease which they have inherited and which their invalid habits have made it impossible to avert. Of the nervous constitution itself very few if any die ; some remain miserable invalids all through life, till some casual disease, aggravated by their

¹ A remarkable case in a child fourteen months old is related by Mr. Thomas Smith, *Trans. Clinical Society*, 1875. I have seen a well-marked mimicry of disease of the knee-joint in a man of sixty-three.

general feebleness, removes them ; but the majority get well ; they live through the time of life in which the constitution is most marked, and then their nervous systems become more tranquil, balanced and orderly.

I think there is no constitution in which nervous mimicry is so commonly found as in patients who are said, and perhaps truly, to have a very bad circulation. Chiefly these are marked by being habitually cold-footed. In nearly all weathers and all times their feet feel no warmer than the air ; indeed, they feel colder, and are often damp and, in those who are not anæmic, are purple about the sides of the toes and under the nails. Sometimes the hands are habitually in the same condition, and the skin on the back of the upper arm is usually dusky, pink and purple, coarse and papillary.

Doubtless this coldness tells of very slow movement of blood in the skin of the cold parts ; and, in seeming agreement with this, the heart is commonly feeble, irritable, and swiftly acting, so that the pulse is very quick, while the breathing is comparatively slow. Often, the inconsistency of a quick pulse with only natural or slow breathing and a low temperature may nearly suffice to tell that some very painful disease of long standing is only 'nervous.'

But, besides, this habitual or very frequent coldness of the feet probably indicates a contraction of the small blood-vessels dependent on disorder of nervous supply. For the phenomena are very variable. Some patients have such feet as, they say, cannot be warmed : they are cold all night, cold while long wrapped in warm flannel ; and even when warmer they may become cold under mental influence. But in some the feet, after being cold all day, flush in the evening ; in others they become red and even painfully hot, puzzling the student of diagnosis.

Such variations in the state of blood-vessels in any

part seem to tell of nervous disorder over-running from the cerebro-spinal into the vaso-motor nervous system. They are weighty evidences in favour of nervous mimicry in any doubtful case; and they become more so when, however often and extremely widely the supply of blood may have varied, the nutrition of the part remains unaffected.

The temperature of a patient in whom you are doubting between real and mimic disease should always be observed. Speaking generally, it is not affected in any degree proportionate to the signs which may seem like those of acute disease. With a joint or a spine as painful as in the most acute inflammation, there is usually a constantly natural temperature; and so of other nervous disorders imitating inflammations in other parts. And this inconsistency may settle your doubts. But if the temperature be variable, or often high, you must be cautious. You may rely much on heats and chills, on shiverings and sweatings, as signs of true disease in parts, and as very probably signs of suppuration; yet they are not quite sure. Many sensitive persons shiver on very slight provocation; for instance, when they are in pain or anxious, or what they call bilious, or exhausted. In hysterical persons, a shivering may take the place of an ordinary hysteric fit. Weakly persons, by whatever weakened, sometimes perspire very freely at night; and mere nervous excitement may raise the temperature to at least 101° . I have seen a case in which, for nearly a month during convalescence after fever, the temperature rose two or three degrees every night; the patient had some signs of joint-disease, but the end of the case showed that there was no real morbid process present. And it may often be observed that, though with little or no organic disease, a nervous patient's temperature may be normal or not above 101° , yet with a moderate addition

of acute disease the temperature may rise much higher than it would in anyone with a healthy nervous system. I perforated an abscess in a very hysterical young lady's tibia; a few days afterwards the escape of pus was casually hindered, and in that evening her temperature rose to 105° . In the next evening it was 104.1° . In the following morning it was 100.5° ; in the evening 101.5° ; and then it fell nearly to normal. And this had happened without any material pain or inflammation; and, even when the temperature was 105° , she was cheerful and with a pulse about 100. Her respiration was natural.

These facts may be enough for caution against over-reliance on any one sign of disease in patients of nervous constitution, even though it be the measurable temperature. Prudently estimated, it is of the highest value, even in nervous patients; over-estimated, it is more fallacious in them than in any others.

In all nervous constitutions, especially in those with the habitual coldness of the hands and feet of which I was just speaking, it is common for many of the internal functions to be torpidly and scantily performed. The bowels are often inactive, sometimes marvellously slow, the gastric digestion is feeble, and menstruation is scanty and irregular or altogether suspended; but in these things there is no rule: in some nervous mimics the functions of organic life are done well enough.

In the defective ovarian and uterine functions of certain patients some see the centre and chief substance of the whole disease: a very mischievous fallacy. Of course the sexual organs appear generally in fault to those who are rarely consulted for the diseases of any other part; but in general practice they are, in a large majority of cases, as healthy as any other parts are, or not more disturbed. The close and multiform relations

of the sexual organs with the mind, and with all parts of the nervous system, are enough to make the disorders of these organs dominant in a disorderly nervous constitution; but their relation to 'hysteria,' or to 'neuromimesis,' though more intense, is only the same in kind as that of an injured joint or an irritable stomach. All, in their degrees, may be disturbers of a too perturbable nervous system; and equally on any one of them the turbulence of a nervous centre may be directed with undivided force.

In patients with very strongly marked nervous constitution, a mimicry of organic disease may ensue as if spontaneously—without provocation. And the chances of such an event are greatly increased in the weakness of convalescence after acute disease, a condition in which whatever is wrong in any constitution has the best opportunity of appearing in force. But much more generally the mimicry begins after some event which has the relation of an exciting cause, determining in many instances not only the occurrence of the mimicry but its locality.

These exciting causes may, then, be our next subject; and these also may be studied chiefly with a view to diagnosis.

Among the chief are sudden mental distresses, emotion, disappointment, long anxieties, or exhaustion by overwork. When any of these can fairly be traced as inducing a seeming organic disease, the probability of a neuromimesis is increased. Sometimes the effect of mental strain is very striking. I saw one day a young gentleman who had been over-working for a civil-service examination. After a three hours' mathematical cram he fainted, and when he rallied had a very close mimicry

of paraplegia, which lasted many weeks. On the same day I saw a gentleman who had been greatly overworked in a prosperous business. He kicked his great toe severely, and had a mimicry of tetanic convulsions in the limb, with night-panics and other curious nervous symptoms, which after a few days were followed by the sensations of spinal disease such as one of his brothers died with. This patient was supposed to be strong, but was really very nervous, timid, and watchful. The other was generally calm, active, and vigorous; but one of his sisters had had severe hysteria and apepsia.

In cases such as these the mental disturbance appears as an exciting cause; in others of longer duration, as with constant anxious watching or any feelings long suppressed, if the mental states may not be regarded as exciting causes, they so greatly enhance the tendency to nervous mimicry that its outbreak may seem spontaneous.

In another form, a mental condition may be regarded as the determining or exciting cause—namely, when a patient imitates unwittingly and unconsciously another person's disease. In a case which I do not doubt was a neuromimesis of hip-joint disease, with limping, and with eversion and contraction and some pain of the thigh, I found that the patient's brother had advanced true hip-disease—a fact which rather added to the difficulty of diagnosis, for with the probability of similarity of constitution in two brothers it might be deemed very probable that both would have the same organic disease.

Similarly, through sympathy, even without family relationship, one person may acquire the sensations of disease described by another. In the fortnight following the death of the late Emperor Napoleon, I was consulted by four persons who described, as they felt, the sensations

of stone in the bladder. One had some slight disease in the bladder; the others were healthy men, except in that they had nervous irritable bladders; but there was not any ground for a suspicion of stone in any of them, and none of them had suspected it, or felt the sensations that led them to the suspicion of it, till they were excited to attention and suggestion of these sensations by the constant thinking and hearing of what were supposed to be the sensations of the Emperor.

More frequent probably than any mental state, among the exciting causes of nervous mimicry, is injury of any kind, especially of bones and joints. In the majority of the cases of mimicry in these parts, and in the spine, injury is told of as the cause of disease. The history thus adds to the difficulty of diagnosis; for injury is as often the cause of real disease—and after injury, let me tell you, nervous mimicry is not only more difficult to be sure of, but harder to cure. For there is something tangible to appeal to, something which would indeed be quite inadequate to explain any severe symptoms in a person of sound nervous system, but which the mind and mimicry can invest with symptoms enough for even the gravest disease.

In many cases you will find it very hard to tell what has determined the locality of a mimic disease. If it be not injury, it may be some inherited local disposition to disease, or a special local excitability; but when we cannot even guess, there may yet be no ground for doubting that the malady is mimetic, for we are in only the same difficulty as we often are in trying to say why gout is in some persons localised in the hand, in some in the foot, in some in the skin, or stomach, or bladder. In all alike the constitutional malady may be clear, however obscure may be the conditions determining its local evidence.

I say the constitutional malady ; and let me again and again repeat, that in every case of this kind you are to look, for the essence of the disease, to the general condition of the nervous system. There is no greater fallacy than to suppose that nervous mimicry, or hysteria, or any of the allied forms of disease, can be referred to any malady of any other part than the nervous system. There is not an injury or a disease of the ovaries, or prostate, or any other part, to which hysteria, hypochondriasis, or any allied malady has been referred, which may not be found, in some persons, free from all nervous complications whatever. It is only the hysterical by natural constitution who can be made to display hysteria by any local disease or injury—only those who are by constitution predisposed to nervous mimicry in whom such mimicry can appear.

LECTURE III.

ON NERVOUS MIMICRY (NEUROMIMESIS) OF DISEASES OF JOINTS.

I have said all that I think necessary, for help in diagnosis, on the general conditions of patients in whom mimicry of disease occurs, and on the events which appear as exciting causes of the mimicry. I will now speak of some of the special mimics, beginning with those of diseases of the joints, and still trying to teach, almost exclusively, their diagnosis.

Let me repeat the rule that, in every case, you should study the local before the general symptoms. You will often go wrong if you take the contrary course. In the most evidently hysterical patient there may be a real joint-disease ; in the least evidently there may be a

mimicry: and the error of treating a real disease as 'only nervous' is one of the worst that can be made; it may be as bad as that of amputating a limb for a mere mimicry of disease.

Among the diseases of joints, those which are most often imitated are the more or less acute inflammations; not, unless very rarely, the rheumatic or gouty or any of that class, but the so-called common inflammations, and the scrofulous. If you would think of the importance of studying them, remember that Sir Benjamin Brodie, to whom more than to anyone else of any time we are indebted for knowledge on these subjects, said that, 'among the higher classes of society, at least four-fifths of the female patients who are commonly supposed to labour under diseases of the joints labour under hysteria, and nothing else.' This statement, of course, does not refer to the class of gouty and rheumatic diseases of the joints; and it needs, I believe, an amendment by the omission of the words 'and nothing else,' for part of the large proportion is made up by numerous cases of trivial disease or injury made to seem severe by hysteria or other nervous fault. The words 'higher classes,' also, need strong emphasis: for among my hospital-patients, whether in- or out-patients, I think I can be sure that the proportion of nervous joints was less than one-fifth; and even in private practice the proportion of four-fifths is not attained unless in practice almost exclusively among the most cultivated classes.

Among all the joints, the hip and the knee, which are the most frequent seats of real disease, are equally so of the mimicry: a fact not easy to account for. It may be due to mental association, perhaps unconsciously; or to a mingled inheritance—for instance, to an inheritance of nervous constitution and of relative weakness in the joint

or joints most weak in progenitors. After the hip and knee follow, in order of frequency, the tarsal joints and carpal, or the elbow and shoulder; but in these, mimicries are too rare for counting.

Another fact, adding to the difficulty of diagnosis, is that the most frequent exciting causes are the same for the real and for the imitated affections of the joints. The imitations, like all the similar nervous affections, may sometimes be traced to mental fatigue or anxiety or worry; but more often, an injury or over-exertion of the part is referred to as the source of the mischief. The injury may seem inadequate for such trouble as followed; but you cannot rely on this. Many of the worst instances of scrofulous arthritis follow injuries that seem very trivial. It may suggest a suspicion of neuromimesis if pain set in with full severity directly after an injury that was not severe; but the suspicion must be lightly held. One of the most acute inflammations of the hip-joint I have seen set in severely, with an almost agonising pain, directly after a wrench of the joint in quickly turning round.

Thus, then, you can get little help for diagnosis from either the seat or the apparent cause of the malady: they are for the most part the same for the real and for the mimic affection. Your reliance must be on the examination of all the features of each case, and on a right estimate of the weight to be attached to each. Let me then take, in turn, each of the signs of inflammatory affections of joints, and see how far they may be imitated, whether separately or together. For although it may be said, generally, that an inflammation of a joint should be marked by many signs, and that in a well-marked case you may study them all, yet, in practice, you cannot treat lightly any case which has even one clear sign of diseased

joint; for this may be only the first sign which others will follow, or the last which has survived the rest; or it may be one which is so exaggerated as to conceal the others. What is wanted in every case of suspected disease is, that you should be able to say positively Yes or No; and this you cannot do without knowing the weight in evidence of each usual sign.

First, as to pain. Alone, it is not to be at all relied on for a sign of inflammation of a joint; especially if it be severe. If a patient be ready to scream when the accused joint is touched, and yet the joint is not over-warm and the patient not feverish, you may be nearly sure of neuromimesis; and more nearly still if the pain be rather in the parts outside or about the joint than in the joint itself, so that a gentle touch is said to hurt as much as a hard one, or a pinching of the skin as much as pressure on the joint itself. You must not even rely on what are supposed to be characteristic pains, such as those felt at the knee for disease of the hip, or about the middle of the arm for disease of the shoulder, or even those grinding and burning pains at night which some regard as characteristic of ulceration of cartilages: all these may be mimicked.

I had a boy aged about fifteen in the hospital who had these night-pains in a most marked degree in one knee, and the joint was a little swollen; and he, being tuberculous, was wasting, hectic, and very ill. I did not doubt that he had destructive articular disease, and the use of the actual cautery cured his night-pain—another reason, some would have said, for being sure that his articular cartilages were ulcerating. Soon after this he had similar pains in or near the tibio-fibular joint, and these also were cured with the cautery. But he went on and died with pulmonary tuberculosis, and I examined his

knee-joint and found it almost healthy. A very small piece of one edge of the cartilage on the femur appeared eroded, but it was a trivial change, and quite inadequate to account for the severe pain of which the lad had long complained.

The case had been one of nerve-pain at the joint, and the imitation of organic disease had been made closer by the signs of the coincident disease of the lungs. But for this, it might have illustrated what you may take as a general rule—that acute pain in a joint, if it has existed for even a few days without either local or general increase of temperature, is not a sign of acute inflammation of the joint. It may be rheumatic disease or rheumatic gout, or some slight inflammation after injury in a neuralgic person, but it is not acute inflammation.

There is more difficulty in judging of the meaning of pain in a joint when it is not severe, but dull, aching, ‘wearing,’ as patients describe it. You must be cautious in these cases. Pain alone is not enough to prove organic disease; yet the lower degrees of pain seldom exist constantly and long without some organic mischief. I have indeed known several such cases, especially of pain at the hip, the conclusions of which made me believe that the pain was only nervous; for the patients got at last suddenly or very quickly well, without stiffness or other apparent change at the joint, or with a shifting of pain to some other part. But in all such cases you must be cautious and watch for other signs of disease adding themselves to the pain, such as local heat, swelling at the joint, wasting of parts about it, and others that are least dependent on the sensitive nervous system.

The pain I have been speaking of is that which may be felt in the joint even while it is at rest. Different from this is the pain which prevents the free movement

of the joint. It is observed alike in the real and the mimic disease; and you may often judge the pain to be mimicry by its inconsistent severity. If with scarcely any other sign of disease a joint will not permit the slightest movement because of pain, you may greatly suspect the reality of disease; but be very careful not to overlook the signs of increased pain on movement in cases of slight real disease, especially in the diseases of the hip in children. You may often find this the only sign making it clear that a child has real hip-disease. A suspected joint may allow free and smooth and painless movement till, for instance, in extreme abduction of the thighs one adductor becomes much tenser than the other, or in extension the loin is quickly raised, or, by some other movement, it becomes evident that the joint will not allow extreme movement without pain, although within all but its extreme range its movements may be free and painless.

In these, among many cases, chloroform or ether may help in diagnosis. In the real disease, as in the mimic, while the patient is utterly insensible, the joint may be moved as widely as in health, unless, indeed, there be such changes in its structure as might alone have proved its disease; but commonly you will observe that, in the real disease, the muscles become alert and restrain the movement of the joint before the patient has regained consciousness: while in the mimic disease there is no restraint till consciousness is completely regained. The test is a delicate one, but I think I may be sure that it is a true one, and fit to be relied on, whenever the chief sign of disease of a joint is a restraint of movement on account of pain and the guarding action of muscles.

Closely allied with this pain on movement of an inflamed joint is its stiffness, with contraction or other set

posture dependent on muscular action ; for this posture, whether it be due to choice or to reflex movement, is the posture of greatest ease, or of safest guard against weight or shock or other causes of pain. The absence, therefore, of the fixed or nearly fixed posture usually observed in a diseased joint may always suggest the suspicion of mimicry. It would be rather strange to see a hip or knee extended after many weeks of such pain as would be felt in an acute arthritis, unless, indeed, they were rheumatic or gouty, with exaggerated pain, or had been carefully maintained in good position. It would require the presence of many other signs of real disease to counterbalance the absence of this sign ; for diseased joints, left to themselves, will be habitually or always in the position easiest to the patient.

But the reverse of this is not true. Very commonly a joint mimicking disease assumes the posture of disease—assumes and maintains it stiffly in even an extreme degree. This may be seen even when there is no objective pain in the joint ; but much more, when the joint is a little really painful. After a blow or strain the nervous condition of a patient may either make the pain so intense as to demand the position of greatest ease, or may bring about this position for the relief of even a little pain. Especially the posture of hip-disease is apt to be imitated by the drawing up of one side of the pelvis and rotating it, so that the limb looks shortened.

The conjunction of pain and stiffness in a joint always looks like real disease ; but you may generally detect the mimicry by observing that, while these things would indicate disease of much severity, everything else is as if there were no disease, or at most only a very mild arthritis. The inconsistency of the several parts of the case exposes its true nature. Lately I had to see a young

lady, with a reported healthy nervous system, whom I found lying in bed with extreme contraction of the thigh towards the pelvis, pain at the hip and knee, increased pain on any touching near the hip-joint, and especially great pain and tenderness at and about the tuberosity of the ischium. She could not bear the least attempt to straighten the joint, and the contraction was said to be persistent during sleep. She had had vomiting, hated food, and looked ill and distressed; and all these troubles were ascribed to slight injury or over-fatigue a few days previously. The case looked very like real disease of the hip; but if it had been so at all it must have been a very acute disease, swift and severe, such as should have had attendant fever. Yet the pulse and temperature were natural, and there was certainly nothing in the case which might not be explained by mere nerve-disturbance. And the event proved that there was no organic disease, for after a few days' rest, with careful food, and some wine, and mental quietude, the pain ceased, and then very slowly the limb resumed its natural power and posture, and the young lady married and is well.

Let me here tell you of two or three notable groups of cases of pain and stiffness of joints without real disease. None are more frequent than those of boys from 10 to 15, who complain of pain and declare themselves unable to walk after injuries of the ankle or knee, or who disuse their arms for pain after injuries of the elbow or other part. They describe the pain as horrible, and hold their limbs in some unnatural position, and limp and cry when you try to make them walk; yet you can find nothing wrong in the shape, or size, or temperature of the joint, or in the general health. It is very hard to say whether they are shamming or neuralgic; but the utter inconsistency of their cases proves that they are the one or the

other, and you must make them use their limbs. They will often do it better if you have first moved the joint severely for them and 'loosened' it.

Something like these, yet different in having little or no pain, are the children who mimic a disease of a joint through fear. After a hurt the immense care bestowed on a joint, and the earnest injunctions not to move it, seem to impress some children's minds so deeply that, long after all is well, they hold up their joints scrupulously and timidly, and dare not try to use them. You may amuse yourselves with the astonishment with which both child and parents find, on a positive assurance, that walking or any other use of the lame limb is quite easy and painless.

Another group allied with these is that in which you find young people with joints contracted by involuntary and quiet muscular power after injury. The joints are painless unless when great force is used at them; and you can easily feel that their stiffness is not through inflammation or adhesion, but through muscular resistance like that which sometimes produces wry-neck directly or very soon after a blow. You feel a kind of elastic recoil at efforts to move them, as if the effort were resisted by a tough elastic substance. Ether or chloroform decides at once the diagnosis; as soon as the patient is insensible the joint becomes movable as widely and as smoothly as an uninjured healthy one, and only slowly if ever stiffens again.

Just like these are the ordinary cases of painless stiff joints in hysterical girls: the muscles hold them fixed, and that is all; the joints are healthy to the touch and the sight, and even to the patients are painless unless violently used.

In all these cases the ground of diagnosis is essentially

the same. You have one or two or three signs of the disease of a joint present in an extremely marked degree, or at least well marked ; a pain, or a stiffness, or both, and limping or other consequent disability, such as would exist in a joint severely or long diseased. But with a joint really thus diseased there should be—unless it had some chronic-rheumatic or gouty trouble—swelling, or heat, or spoiled shape, or general wasting of the limb, or all these together ; and with them usually some disturbance of the general health. The absence of these is weightier evidence than the presence of the other symptoms.

All that I have just been saying relates to the distortion of joints produced by muscular action, not to deformity due to displacement of one or more of the bones forming the joint, such deformity as you see, for instance, in the knee, when the tibia falls backwards and outwards from the femur, or is rotated by the weight of the foot, on which also the bedclothes may have been allowed to rest. When this or any similar deformity exists, it is a nearly sure sign of real disease, past or present, for it can scarcely happen except through textural changes, through softening of the ligaments and other structures about the joints permitting one bone to move away from the other. Now such softening as this can hardly take place except in inflammation. I will not say that it is impossible, but I know that it is very rare. Posture alone, though very long-continued, will not produce deformity of joints with displacement of bones. I saw a gentleman who, in a half-lunatic condition, sat for five years in the same posture, and was credibly said to have never moved. At the end of this time his knee-joints were contracted to a right angle and felt as if absolutely fixed ; yet they were not deformed. Their bones had their right relations ; and,

after some weeks of extension with instruments, the knees were straightened, and power over them was completely gained.

So, too, in a case told me by Professor Flower. A man, whose skeleton is at Marburg, was encased by his relatives for twenty years in a space in which he could only sit with his limbs doubled-up, and in which he could have had only very narrowly restrained movements of his joints; yet his limbs did not become deformed, and his joints retained their normal textures. And many a case of hysterical joint, after being contracted for years, has recovered without any error of shape.

Speaking generally, then, the presence of deformity in a joint reputed diseased may be taken as certain evidence that it was or is diseased, and the absence of such deformity in a joint which has been long very painful or, in other characters, has seemed acutely diseased, is nearly as certain evidence that there has not been acute inflammation; unless, indeed, the shape of the joint has been maintained by careful treatment. A few weeks of acute inflammation of a joint will almost certainly change its shape and the relations of its bones, unless care is taken to prevent the change; but even many months of mimicry of acute disease will not produce or permit such change.

LECTURE IV.

If it seem strange to you that I should devote two lectures to the subject of the mimicry of diseases of joints—a subject which is usually dismissed with a few lines,—let me say that I estimate its importance by what I know to be its difficulty. I rarely pass a week without seeing at least one very doubtful case, in which a diagnosis

cannot be made without a complete consideration of all the symptoms discussed in the last lecture and of others to be spoken of in this. For difficulties such as these, two lectures may be tedious, but they are not superfluous.

Let us, then, go on with the symptoms of diseases of joints, and see how the mimic may be distinguished from the real. And the next shall be the wastings of limbs near the affected joints. This wasting occurs quickly in nearly all acute inflammations of joints; more slowly in the chronic inflammations. In the chronic it may be from disuse alone: in the acute it is not so; for it is much more rapid and extensive than in any cases of mere disuse. Compare, for instance, cases of fracture of the thigh with those of acute disease of the hip, and you will easily see how much greater is the wasting with acute disease than with disuse alone. Rapid wasting may be seen in the whole lower limb, especially in the thigh and the nates, in all acute hip-joint diseases; more slow wasting in the painless scrofulous diseases; less in the chronic rheumatic. Wasting in the lower part of the anterior and lateral muscles of the thigh is quickly evident in acute knee-joint disease; only less quickly in the chronic. In similar affections of the shoulder you may see it in the flatness of the deltoid and of the muscular coverings of the scapula; and I think that the same wasting occurs, in greater or less degree, in all muscles near joints that are inflamed; and the more quickly, the more acute the inflammation. It is, I repeat, not a mere wasting from disuse: it is far more rapid than that; more like what has been called acute atrophy of muscles, such as may be seen in the swiftest cases of infantile paralysis; and it is, I think, very rarely if ever so well repaired as is the wasting from disuse.

This process of wasting is one of singular interest in

pathology. I wish I could explain it better than by calling it reflex atrophy. It seems dependent on disordered nervous influence, and often appears proportionate to the coincident pain, as if it were due to the disturbance of some nutritive nervous centre, irritated by the painful state of sensitive nerve-fibres.

But, however the wasting may be explained, it is, unhappily for our present needs, not certainly diagnostic of real joint-disease. You may find it nearly as marked, though not so quickly progressive, in some nervous affections as it is in acute inflammatory joint-disease. I say you may, not you will; and I cannot tell you in what nerve-cases it will be found, in what be absent. I believe it is in inflammations of nerves or in inflammations involving spinal ganglia; but I cannot tell. However, as to diagnosis, you may find inflammation of the hip-joint imitated, so far as pain and wasting are concerned, by some painful affections of the sciatic nerve; inflammation of the shoulder-joint by painful affections of parts of the brachial plexus; and, more frequently, the wasting at the lower part of the thigh, which is common with acute inflammation of the knee-joint, is closely imitated in the cases of certain knee-joints which are painful but not inflamed. In the cases of this kind that I have seen there was no sign of inflammation besides the pain; no heat, no adequate swelling—if, indeed, there were any at all; the patients were nervous or hysterical, and at the end the joint though wasted was unchanged in shape and structure.

You may hold, then, that, generally, the wasting of the muscles about a suspected joint adds to the suspicion that it is or has been inflamed; but you must hold this cautiously. You must expect to meet with cases, however rarely, in which similar wasting attends pain at a joint

without inflammation. But, all the more because of these cases, you may hold that if a joint has long been very painful, and yet there is no wasting of the muscles near it, it is not inflamed.

Let me tell you, by the way, that wasting at the knee is commonly produced and is always aggravated by the wearing of elastic knee-caps or tight bandages. I have often been surprised to see how quickly and to what extent these pressures will produce wasting of muscles and weakness, aggravating all the trouble consequent on injuries and diseases of joints. They will in this way do such damage, that, except for comfort during active exercise, or for the purpose of reducing chronic thickenings and collections of synovial fluid in joints, I believe they ought never to be used.

Wasting, then, can be only cautiously judged as among signs of real joint-disease; it is too common in mimicry to be a safe sign of reality. And so is another sign; impaired utility, or awkward use, such as we have to observe most often in limping or other manner of lameness.

The use to be made of this sign in diagnosis is as of pain. It may be absurdly exaggerated, caricatured; and by mere excess may prove itself unmeaning, as when a patient in good general health, and with a cool or cold well-shaped foot, has maintained for many weeks that it is impossible to bear weight on the foot; or when one, in whose knee you can neither feel nor see any wrong, goes limping as if with an utterly ruined joint. Here, as in many other cases, inconsistency proves unreality.

The difficulty of diagnosis is far greater when there is only slight limping or other impaired use of a joint. Here you had better be very watchful and cautious, and err, if at all, on the side of believing in real disease; for the

cases are frequent, especially in children, and in serious diseases of the hip, in which the first, and for a long time the only, sign of real disease is some limping or other erroneous use of a joint.

As you watch the cases in which limping or some manner of lameness is the chief or only apparent sign of disease of a joint, you will find that some depend on, or are ascribed to, mere muscular weakness of the limb, some on a partial slight chorea. The former are often associated with what Sir B. Brodie pointed out as sometimes occurring in hysterical persons—a peculiar laxity or limpness of joints. These are not difficult of diagnosis; if a really diseased joint is loose the disease is very plain. The choreal cases, among which I have seen one which appeared due to ascarides, are more likely to deceive. In some there is a sort of string-halt—a quick jerk-up of the heel at every step, faintly suggesting some affection of the knee-joint. Much more puzzling are the cases of slight chorea of the whole lower limb in which, as a patient walks, he limps, and jerks, and throws-out his leg, somewhat after the manner of one with early disease of the hip. The likeness is, indeed, not very marked; yet in two cases that I have seen it caused great fear: in one, because of the importance of the patient; in the other, because the limping followed a blow, and a brother of the patient was crippled with serious scrofulous disease of the hip. In this case, too, the ordinary posture assumed in standing was—by imitation, I believe—like that of diseased hip, with the half-pelvis raised and the foot pointed. The diagnosis of such cases may rest on these facts: if the other usual signs of diseased hip-joint are absent or very little marked; if the limping movements are not careful but quick and jerking; if the passive movements of the joint are complete and free; if when

the patient stoops, so as to touch the feet with the hands, the figure becomes symmetrical; there is no real disease. And this will often be certified by choreal movements, such as twitchings of the face or eyelids, told of or still present.

I have thus gone through the chief reputed and usual signs of inflammatory diseases of the joints which may be imitated by nervous affections closely enough to make a diagnosis difficult. But other signs remain which are much more rarely imitated, and never closely, except in some cases of complication of nervous affections with fever or other accidentally concurring diseases. These are swelling and local heat and fever.

Now, as to swelling of the whole or part of a joint, its absence may be nearly enough to prove that a joint in which there is intense pain or other sign of acute disease, or which has been long painful, or in any way seemed long diseased, is only nervous. Inflammation of a joint, either very acute or of long standing, can hardly be found without visible or tangible exudation in the joint cavity or in the textures bounding it. But there are many cases in which you cannot apply this rule for diagnosis. A joint shrunk after long disease may relapse into inflammation without renewed swelling, till perhaps a residual abscess appears. A hip-joint may be acutely inflamed without any discernible swelling; so, less often, may be a shoulder, the exudation being too little to be felt. And, making more difficulty, swelling is sometimes evident in a merely nervous joint; not indeed considerable swelling, but enough to make a mimicry of real disease much more close. You may often see this in the loose tissue by the sides of the ligamentum patellæ. The swelling sometimes appears due to such slight exudation as may ensue in any neuralgic part; like the puffiness

that may come on in facial neuralgia, or the swelling and congestion of the conjunctiva in some cases of orbital neuralgia. Such swelling is commonly transient and capricious, and the fallacy may be detected by observing that, at its greatest degree, it is not, even after long time or many repetitions, nearly proportionate to the pain or duration of the disease. For a joint which is intensely painful, with acute inflammation, or with ulceration of cartilages, should be, if not at first, yet soon after, considerably and always swollen.

In other cases, swelling of a nervous joint may be due to accidental conditions. For instance, repeated blisterings or repeated paintings with iodine will give, for a time, such thickening and puffing of the subcutaneous tissue about a joint as may be very deceptive when added to the other signs imitating inflammation of the joint. So, again, I have seen such a condition in a very marked degree produced by the long-continued use of ice about a painful joint.

On the whole, then, the absence of swelling makes it very unlikely that a joint is really diseased; so does the presence of only a trivial swelling when the nervous and muscular signs of disease are acute or of long standing; and when swelling exists it must not be counted as adding to the probability of real disease, unless it persists and is independent of such accidents as I have already named. And remember that a sensation of swelling is not unfrequently complained of when no swelling whatever exists. It is just one of the erroneous sensations to which nervous persons are prone, as they are to sensations of unnatural fulness and of weight for which there is no substantial reason. Such a mere complaint of swelling will not deceive you if you compare the suspected joint with its fellow.

But, after all, the sign most to be relied on for diagnosis between real and nervous disease of joints is the temperature. It is so important to estimate it accurately that I cannot too strongly urge you to be always studying it. You should feel with a broad surface of your hand every joint very watchfully, comparing each that is supposed to be diseased with its fellow supposed or known to be healthy, till you learn, as you certainly may, to detect even a small difference of temperature in even a small part of a joint.

When you have learnt to feel accurately the heat of a joint, you may safely rely on it for some certainties in diagnosis. A joint that feels all over cold, or cool, or not above its natural temperature, is not an inflamed joint: whatever may be the other signs of inflammation in it, it is not inflamed; you may rely on this. In the hip and shoulder, however, this test of temperature is not easily applicable. The thickness of various textures covering those joints is too great for additional temperature to be transmitted through them. But in the knee and elbow, and other smaller joints, even those of the fingers and of the tarsus and carpus, the test is easily applied and sure; and remember always, in using it, that in most persons the front of the knee-joint, and, in a less degree, the back of the elbow, are habitually cool to the touch—cooler than the arm and leg; and that in many the feet are rarely warm in health. These local variations, however, can lead to no confusion if you follow the rule—which, for other reasons as well as this, ought to be never neglected—of comparing every suspected joint with its fellow. Besides, always compare the temperature of the joint with that of the rest of the limb, for the rest of a limb may be, through disease or long defective nutrition, cold; and if one joint in it be always not cold, though it

may not be fairly called hot, this may be through its being inflamed. I repeat, then, a joint which is not above its natural temperature is not an inflamed joint. But observe, besides, some rules within this rule.

A joint when first exposed for examination may feel over-warm. This may be due to its having been very warmly wrapped, or thickly covered with water-dressing, or in some other way hindered from cooling to its natural temperature. Wait, therefore, and observe whether, after a few minutes' exposure to cool air, the joint has become of the same temperature as its fellow or as the rest of the limb. If it have, you may be very nearly sure it is not inflamed; or your suspicions that it is inflamed may increase with the time during which its cooling is delayed. Frequently only a part of a joint is inflamed, and in this case the morbidly high temperature can be felt only over the inflamed part. This limitation of a sign of inflammation which one might suppose easily diffusible is a striking fact. It is the same with the swelling, which, especially in scrofulous inflammations of joints, is often limited to a single portion.

Not rarely when you feel a joint quite cool, the patient will tell you that it is, nevertheless, at times very hot or burning hot, and that with this heat it swells and becomes red. Such cases may be gouty; but they are more common among those in which the consequences of sprains and other injuries are very prolonged in nervous persons, or even in healthy persons whose joints have been too long treated with cold douches or too long kept at rest. These joints are commonly reported as getting very hot every evening, and as being subject to painful heats, tingling, and burning. In any such case you may decide that the joint is not inflamed. If it were, it should be over-warm all day and all night. The occasional heat

is only due to flushing, such as some nervous people have in their faces after their meals, or such as women commonly have at the time of ceasing to menstruate. I call it flushing, not blushing, for it is not associated with any mental state; indeed, it may seem strange that, among all the nervous people with joints that are the constant objects of their attention, one never sees blushing of the skin over them when they are being looked at. All such turgescence and transient heat are consistent with complete absence of organic disease. Doubtless the same thing may happen in inflamed parts; they are sometimes hotter than at other times, more swollen and more painful, having periods of exacerbation; but then they are never of natural temperature—they are always more or less too hot. It is very different with the mimicries of inflammation in joints; here the over-heat is only at times, at night or at some nearly regular hour, or after exercise or fatigue. A joint which is cold by day and hot by night is not an inflamed joint—that is certain.

The certainty of the diagnosis based on coldness is, if possible, increased by coincident duskiness of the skin—by its having the dull purplish tint which is commonly called blue or a dull pink. Such colours may be seen at joints long inflamed, but in these cases they are associated with over-heat; when they are associated with coldness, they are characteristic of anything rather than inflammation. Besides, with inflammation the colours of overfulness of blood-vessels are only at the inflamed parts; with congestion in cold nervous joints they are commonly much more widely diffused.

Lastly, as to fever associated with a supposed inflammation of a joint. It may help you to diagnosis in some few cases, chiefly in those in which the pain is very severe, or in which there are other signs like any of those

of acute inflammation. For no very acute inflammation of a considerable joint can exist without fever; and therefore, when a patient's general temperature is normal, you may be very doubtful, to say the least, whether an intensely painful joint is inflamed. But the reverse is not true; the temperature may be frequently or habitually high, though a suspected joint be only neuralgic or in some other mimicry of acute disease. For the patient may be tuberculous or, with some casual illness, may have a high temperature, or he may be recently convalescent from acute fever. Moreover, a very slight degree of inflammation in a joint in a very nervous person may be associated with a disproportionately high general temperature. This is in accordance with a general rule already mentioned—that in those with alert and mobile nervous systems a slight local inflammation may produce or be associated with a great increase of general temperature. In similar persons, fatigue or passing excitement will raise the temperature to 100° or, I think, to 101° ; and in their convalescence from acute illness—a condition in which nervous mimicry, as well as real joint disease, is apt to appear—they have widely variable temperatures. You must therefore hold, as a general rule, that, in very nervous persons, the temperature must be studied many times, and with circumspection, before it is counted as an addition to the evidences of their having real joint-disease. Within the last few days I have been very nearly deceived in such a case. A gentleman, about thirty, had what he described as frightful paroxysmal pain in one knee—pain such as might have been in the most acute inflammation of the joint; and there were some heat, some swelling, and a general temperature of 101° . These things had 'come of themselves' within three or four days, and the patient

looked very ill. Of course he was treated cautiously ; and all subsided so quickly as to make it sure that no serious, if indeed any, organic disease had existed. It was a nervous mimicry ensuing upon excessive and unsuccessful work.

LECTURE V.

ON NERVOUS MIMICRY (NEUROMIMESIS) OF DISEASES OF THE SPINE.

For the next group of cases of neuromimesis or nervous mimicry I will take those of Diseases of the Spine. They are nearly as frequent as the imitations of diseases of the joints ; and their diagnosis, of which alone I shall speak, may be studied in the same way ; weighing the evidence of each of the chief local symptoms, and then adding or subtracting whatever may be gathered from the patient's constitution and the history of the case.

The chief things to study in the spine are pain, stiffness, weakness, and deformity.

As to pain—spineache and backache, often not distinguishable, are perhaps the most common sufferings of the whole class of hysterical and other such nervous patients. Few escape them. But, then, similar pain is so common in other patients besides the nervous, and is found in so many and so various diseases, that pain alone can rarely decide a diagnosis. I must leave out of the question all the 'backaches' that belong to lumbago, rheumatic gout, uterine, ovarian, renal, and other maladies, and try to answer for you only this : Are there any characters by which we may distinguish between pain that signifies real disease of the spine and pain that mimics such disease ?

Well, we cannot always do so. There is no pain which is characteristic of real spinal disease—no manner of pain which may not be closely mimicked; but, in many cases, the nervous mimic-pain has characters which are not all found together in real disease of the spine.

Nervous spineache is often described as very severe—raging, racking, as if the back were breaking, and so on. Now, such pain as this does not occur in real spinal disease, unless when the pain of some slight disease is immensely exaggerated in a nervous constitution, or in the acutest form of inflammation of the vertebræ—a very rare disease, always associated with serious general illness and impaired mobility of the spine. If there be no considerable attendant illness, an intense and horrible pain in or at the spine does not mean serious disease of the spine. I do not say that such pain is always merely ‘nervous’ when it is the only wrong complained of. It may be due to gout, or to aneurism, or cancer, or to some distant malady in a nervous constitution; but, unless in the cases of aneurism or cancer, it is not a sign of serious disease involving the spine.

And this negative becomes the stronger when with such a pain there is excessive tenderness of one or more parts of the spine. Some tenderness on pressure of spinous processes may be found with real disease of the spine or cerebro-spinal membranes; but excessive tenderness is not, unless in the rare instances of periostitis of one or more spinous processes, which are usually associated, I think, with syphilis. It is rather characteristic of merely nervous disorder, of the so-called spinal irritation, and usually you find it, not at one, but at two or more parts of the spine, most frequently between the scapulæ and at the loin. At these tender spots the nervous patients cannot bear to be touched; they flinch

and writhe when the finger taps or presses them very gently. You may be nearly sure that there is no disease of the spine when you see this, or when the tender parts of the spine are not painful in moving or in coughing or sneezing. And you may be quite sure, I believe, when a gentle blow or pressure produces more pain than a hard one, or when you find the same pain or flinching if the skin over or near the spine is pinched without pressing on the spine itself.

Again, the merely nervous pain is usually variable, though it may never be wholly absent ; and its variations seem to be more dependent than those of real diseases are on disorders of distant organs, as the ovaries or uterus, the colon or rectum. In these variations the pain may seem paroxysmal, but I think it is not often so of itself.

Another usual character of this pain is its frequently extreme increase in fatigue, not only in bodily fatigue, as in long sitting upright or standing, but in long occupation of any kind, even in mental occupation if it is not very agreeable. With this character, also, it is common for the pain to be accompanied by nausea or even vomiting, and it is a nearly sure sign of merely nervous disorder if pressure on the spine produces shivering or nausea or a feeling of going to be sick.

And, lastly, it is a sign of nervous pain alone if the pain has lasted many weeks or months, and nothing has come of it ; no immobility of the spine or ribs, no paralysis, no fever or wasting or great disturbance of the general health.

I think you may be sure that a patient in whom you find all or most of these characters of pain in the spine has not spinal disease. But you will find many with pain like enough to this to excite suspicion, though not

nearly like enough for conviction ; many in whose cases the diagnosis must rest much more on other signs than on that of pain. Among these, let me warn you that the greater danger is that of thinking cases to be mimicries of disease, or 'muscular' or 'only rheumatic,' when they are really serious organic diseases of the spine or of parts within it. I will try to diminish the danger of being wrong in either direction by mentioning some of the more frequent groups of cases difficult of diagnosis.

The curvatures of the spine that occur frequently in young persons are often painless, are seldom very painful, and have no characteristic pain : yet pain of any kind should always lead you to examine for curvature, and to suspect, if there be curvature already, that it is increasing. And the same rule should be observed with older patients ; for it is not extremely rare for lateral curvature to begin after middle age, nor at all rare for it to increase at any period of life. In either case there may be pain, for which the spine should be often looked at for sign of disease more characteristic than the pain alone can be.

Aching spines are common in men with very nervous constitutions—in such as might be called hysterical with as much justice as many women are. Especially they are common in such men from thirty to fifty or more, and in women who retain their nervous or completely hysterical constitution to the middle or later times of life, and in whom, very probably, it is mingled with gout. There are far more cases of this sort in the cultivated classes of society than you are likely to suppose : men and women of mature or post-mature age, with spines whose nervous aching disable them from active life—at least from active bodily life ; for in some of them the mental life is as active as if it really took their whole

nervous force for its own use. They suffer severely, are painfully fatigued with exercise, have no comfort unless when they are lying down or reclining with their backs firmly supported; even slight manual work tires them: and yet in some of them the mind seems incapable of fatigue, or it only tells its fatigue in some increase of their spineache, or in feelings of nausea or vomiting. In many of these cases the diagnosis is not difficult: the nervous constitution is very strongly marked and, after months or years of weariness and ache, you find the spine as straight, strong, and pliant as ever.

And yet among these are the chief cases in which you must be on the watch, lest you overlook a really diseased spine; for, among elderly persons, you may not only, as I have said just now, find cases of beginning or increasing lateral curvature, but, occasionally, one of disease leading to angular curvature, setting in very insidiously like a case of nervous pain or chronic rheumatism. The first sign of this is, usually, a constant aching, which is sometimes even severe, fixed at one part of the spine, or radiating from it, or extending round the sides; and there may be some tenderness. But, chiefly, you may know them by the diminished mobility of the affected part of the spine and of the adjacent ribs. The patient holds the affected part of the spine as still as he can, when he turns or bends. If he coughs he does it carefully, and sneezing is very painful. If the dorsal portion of the spine be affected, the corresponding ribs scarcely move; if the cervical, pressure on the top of the head is often painful. After some few weeks of such pain, yielding of the spine may become evident. If the patient is the first to notice it, he feels himself stooping, or obliged to support himself with his hands, or not so tall as he was; and, when you strip him, you may find two or

three vertebral spines projecting in an incurable angular curve.

These things, you see, are much the same as those by which you recognise the progress of many cases of caries of the spine in children and young persons, and I suppose that this disease in elder persons is of the same kind. But I have not yet seen a fatal case, or one examined after death; and I have seen only one in which abscess was associated with the angular curvature.

You may make another group of cases from other patients, in whom a strain of any part of the spine is followed by a very long-abiding pain. Some of these will tell you that at the strain they felt, or even heard, something crack, and that they have had pain and weakness ever since—aching, wearing pain, increased by exercise, or in certain postures. Here again I must advise caution, especially when the injury is recent. In a large majority of these cases, the long-abiding pain and other signs of spinal disease may be referred to the same nervous mimicry as we see in those whose sprained knees or ankles remain for weeks or months painful and weak, yet without disease. But in some the pain continues because, as may happen in an injured joint, the injury has been followed by inflammation; and this may be of serious form. The pain alone will not prove it; but you may be afraid of it if the pain be felt not only in the spine but in the sides and abdomen, as if in a zone extending round from the painful part of the spine; or if with pain you find diminished mobility of the spine or ribs, great pain in coughing, greater in sneezing; and much more afraid when you find occasional or constant fever, and loss of weight and general strength. In short, for the diagnosis between the real and the mimic diseases following injuries of the spine, you may study by the rules which are more

easily learnt in the similar affections after injuries of the joints.

It is happy for our need of diagnosis that the impairments of the mobility and strength of the spine are more significant than its pains. The spine or any part of it, when really diseased, is often stiff and weak, though other signs of disease are absent or ill-marked; in the mimicry the mobility is often perfect, though other signs are vehement.

Fixity of the spine or of any part of it is extremely rare in any nervous mimicry. I will not say that it is never seen, but it is so rare that when you find a patient carefully—not with muscular spasm, but with care—holding his head and neck or any part of his back very still, turning himself or bending cautiously, anxious not to cough or sneeze, and very careful when he must, then you must look for disease of the spine. There may be muscular rheumatism, lumbago, or inflamed cellular tissue, or abscess, or whatever else may give pain in moving; but there is not likely to be only a nervous mimicry of spinal disease, and there may be the reality.

This fixity of spine is the more significant of real disease the less the attendant pain. Still more so is it when the ribs connected with the suspected and stiff part of the spine are also motionless or too little moved in breathing, and when the breathing is chiefly diaphragmatic. There may in this case be disease within the chest—perhaps the pleurisy that is often associated with acute inflammation of the dorsal part of the spine; or it may be chronic rheumatic arthritis of the spine, or a condition associated with osteitis deformans; but it is very nearly certain—as certain, I think, as any diagnosis of unseen disease—that there is not a mere mimicry. Whenever

you are told of 'spinal irritation,' 'hysteric spine,' or whatever else an unreal disorder at the spine may be named, look carefully to the mobility of the spine and ribs; if it is impaired, look much further before you venture to conclude that the malady is only nervous.

The very opposite state of the spine, in which it is all limp, so that when the patient tries to sit up, he, or more often she, bends or tumbles this way or that, like a baby, is nearly certain to be without organic disease. There may be real paraplegia; if there be not, you may believe the weak tumbling spine is in itself healthy, though it may contain an idiotic spinal marrow; as a good skull may hold very foolish brains.

A feeling of weakness or giving-way at one part of the spine is ambiguous. It is often complained of in neuromimesis; but it is also often present in carious or other real disease of the spine. It must always be considered likely to be a grave symptom if the patient habitually, and almost unconsciously, helps to support the spine with the hands or elbows as he sits. Of course this self-supporting posture may be either unintentionally or on purpose imitated in a nervous mimicry; but it so often has a real meaning that it must not be made light of, unless all other evidences of mimicry are clear.

You will often find that with this weakness of one part of the spine there is some distress in moving it. The patient does not willingly rise or turn in bed, and if he walks he does it slowly, often stooping or leaning forward, going stiffly or shuffling, not staggering or unsteady unless the spinal marrow be involved in the disease. This sign is, I think, less rarely mimicked than the last; the two together are weighty evidence for real spinal disease, and if defective movement of the ribs be added you had better believe that the spine is certainly

diseased ; whether with struma or rheumatism, or whatever else.

The other leading sign for the diagnosis between real and mimic diseases of the spine is in its shape. Is it mis-shapen, wrongly curved, or in any way deformed ? If so, it probably is or was really diseased ; and yet even here is room for error.

An angular curvature of the spine—I mean such backward outstanding of one or more vertebræ as is due to thinning or loss of substance of their bodies or intervertebral discs—is, I believe, quite inimitable by any nervous or muscular condition. But in not a few persons you will find that one or two vertebral spines naturally project a little, or are placed a little on one side of the exact line or curve in which they should be. This is most often seen in the lower dorsal and lumbar part of the spine ; but it is so little like disease that it would be unnoticed if spineache or some fright about curved spine did not call attention to it. I think you will seldom have any difficulty in distinguishing this natural error from any effect of disease.

A lateral curvature of the spine may be imitated by disorderly muscular action ; not, indeed, perfectly, yet near enough to be often deceptive. Spasmodic wry-neck is well enough known, and may imitate the distortion of considerable disease of the cervical part of the spine. Lateral curvature of the dorsal and lumbar parts from similar muscular disturbance or from twitching, like chorea, is much rarer, but you may expect to meet with it ; and you may often detect the mimicry by noticing that the curvature has formed very quickly or even suddenly, and has become marked or extreme in so short a time as could not have sufficed for a real lateral curvature. A few days will make an imitation-curvature stronger than

as many months will make a real one. Besides, the mimic curvature is not a perfect likeness. It is sometimes single, though very marked; real lateral curvatures, if very marked, are at least double; and the vertebræ are little or not at all rotated, as they are in well-marked real lateral curvatures. If these signs of distinction are not enough, you may often find that the spine will become straight and the back symmetrical if you make the patient stoop so as to touch the feet with the hands. In more difficult cases ether or chloroform will help. You can straighten the mimic curvature when the muscles cannot act; you cannot so straighten a real curvature.

Other deformities of the spine may be imitated by hysterical and other such persons. The spine may be rotated without curvature, or drawn to one side, so as to go-up obliquely from the pelvis; or in those with very weak limp spines there may be, when their trunks should be erect, an appearance of an uniform posterior curvature. But I believe you may easily detect the mimicry in all these. Most of them are extravagant, going beyond almost any real deformity of the kind that such a patient could have; and, almost always the wrong may be righted by putting the patient in some unusual position, as lying very flat on the back or front, or with the hands touching the feet, or hanging on a swing. By these, or the like means, you may out-trick the trickery of the muscles.

One thing more: you will often be consulted about irregularities of the scapulæ. Lady-mothers are always watching their daughters' shoulders (happily for sons, their shoulders are neglected), and any unlikeness of the scapulæ, such as one being higher, or further back, or smaller, than the other, is an alarm of curvature of the spine. The alarm is usually premature or false. A

difference between the scapulæ may be due to muscular trick or awkwardness, or overwriting with the shoulder pushed up, or any of several other things; but if you cannot see in the spine or ribs some wrong curvature, rotation, or other mis-shape, the position of a scapula does not prove it. Still, these and the like cases are often hard to advise in, for the higher education of girls is apt to promote all those things which are favourable to the production of both real and mimic diseases of the spine. Wearisome mental occupation; long writing, with the spine twisted and the shoulder thrust up; long upright sitting, fatiguing drill and calisthenics; denial of most natural exercises, and of rests at will, or in any easy posture which is thought ungraceful—these and the like things in the education of girls are equally favourable to the development of the nervous constitution, and to the production of various backaches, and to the deforming of the spine. You must do your best in all such cases to discern what is real, and what is mimic. I can give you no general rules about them.

In trying to teach the diagnosis between the mimic and the real diseases of the spine, I have purposely spoken of only those signs which are usual in the early stages of the real diseases, for it is only in these early stages that the diagnosis can be difficult. A well-marked unalterable deformity of the spine leaves no question about mimicry, unless it be about the nature of some accompanying pain. The appearance of swelling, like a chronic abscess, by the side of the spine or in the groin may not make you sure that the spine is diseased, but at least it answers any questions about mimicry; and so do hectic fever, and habitually high temperature, and constant wasting. These things need no teaching: but I must speak of one more set of symptoms—the paralytic, the losses of sensa-

tion, or more frequently of muscular power in the limbs,—which may be associated with diseases of the spine. I must refer to them, though I believe they very rarely give help in diagnosis. For paralysis due to disease extending from the spine to the spinal cord is rare, except in the later stages of disease, and of these the evidence is clear enough without the paralysis. And when paralysis happens in what you may suspect to be the beginning of disease of the spine, it will very seldom help you, for you cannot tell that which is due to disease commencing in the spine from that which is due to disease commencing in the cord. And, lastly, if you are suspecting a mimicry of disease of the spine, here, again, paralysis may not help the diagnosis, for itself also may be mimicry.

Thus your only surety for right diagnosis between real and mimic affections of the spine is in the examination, first, of the spine itself, and then of the patient's constitution, and in a constant belief that there are some cases in which the diagnosis is very difficult.

LECTURE VI.

NERVOUS MIMICRY (NEUROMIMESIS) OF TUMOURS.

I said in my first lecture that I should treat of nervous mimicry from only the surgical point of view. From even this limited view many things may be seen which I must omit, as mimic paraplegia, mimic tetanus, mimic aphonia, and others. Concerning these you may find guidance to diagnosis in the best works on Hysteria. I will take now a set of cases that have been much less written of—the nervous mimicries of tumours. Of these there are three principal forms: the muscular

or phantom tumours, the imitations of aneurisms, and those of cancers.

In what are called phantom tumours the imitation is due to the swelling of part of a muscle during contraction. This part, swelling and becoming harder than the rest, feels something like a smooth round or oval tumour or cyst placed in or between the muscles. The rest of the muscle feels natural, whether relaxed or contracted. In the contracted state the apparent tumour is most distinctly felt; in the relaxed it may be nearly concealed, as if sinking in the muscle.

This condition is altogether a strange one—not imitated, so far as I know, in any other state of muscle, unless it be in crimping, or in the flickering and passing contractions of dying muscles, or in the lumps which, it is said, may form in those that are struck soon after apparent death in any acute fever. If it may be compared with anything in living muscles, it may be with cramp which draws them into knots or lumps; but there is no pain in phantom tumours as there is in cramp. Whatever it may be, the condition seems due to erroneous nerve-force in the muscles; for one form, the phantom tumours of the abdominal muscles, is most frequent in evidently hysterical women, and in other cases that I have seen there was certainly an abnormal state of the nervous system, and in others it was very probable.

Three cases which I chanced to have seen lately may illustrate the disease.

In one, a gentleman about 60 years old came to me for a tumour, as he thought, in his right calf; and I found in the mid-substance of his gastrocnemius what seemed to be a roundish smooth lump, nearly two inches in diameter. While examining it I seemed sometimes to lose it altogether, as if it slipped away or receded into the

depths of the calf: and then, after a time, I convinced myself that it had no real existence as a tumour; that it could be put-out by friction, or by complete relaxation of the muscle, or in some postures of the limb. It seemed due to defective or disorderly nerve-force in the muscle; for the patient had been slowly becoming very feeble in his lower limbs, and had the slow, short-stepping, shuffling gait, and weak, monotonous voice and unclear speech, which, I suppose, betokened senile degeneracy of nervous centres.

The second case was that of a clergyman, about thirty, who came for weakness of the lower limbs and frequent falling, which he was disposed to ascribe, in some measure, to a 'lump' near his left knee-joint, and another, less prominent, near the right. His calves were very large and muscular; his thighs small and weak;¹ and the lumps were swellings of the lower ends of the internal vasti, when, with the other wasted and weak muscles of the fronts of the thighs, they contracted. They did, indeed, look something like smooth oval cysts by the knees; but they subsided so quickly and completely when the action of the muscles ceased, that there was no difficulty in seeing what they were. And here, too, was nervous defect.

The third case was that of a lady about sixty, who had had a tumour on or over her left parotid. This had been removed about eight years before I saw her, and in the operation the facial nerve was injured, so that the left facial muscles were partially paralysed and subject to occasional twitchings during emotion. In the last year there had been a renewed superficial growth over the parotid, and with this an increase in the facial twitches.

¹ In some of its features this case resembles the pseudo-hypertrophic muscular paralysis of Duchenne.

But what much more alarmed the patient was the appearance of a tumour, as she thought, below the left clavicle. This was the clavicular portion of the pectoralis major, partially contracted and hard, and perhaps made irritable, or more attractive to the alarmed attention, by the frequent twitchings of the platysma. All appearance of tumour was spoiled by raising the arm above the head; when the muscle was completely relaxed it all felt as soft and natural as its fellow.

This last patient was over-sensitive and excitable, and probably augmented the disorderly action of her muscle by her too careful attention to it. The direction of the mind alone may, indeed, suffice to make, in some persons, a distant imitation of a tumour in a muscle. I once saw a gentleman who was possessed with the fear that he would die of psoas abscess because his brother did; and, for a day or more, he was constantly pressing his abdomen towards the psoas by the spine, till a part of his external oblique abdominal muscle had become so irritable that it hardened and swelled at every attempt at pressure and felt something like a tumour. All this ceased when it was completely relaxed by posture. But in ordinary cases the mind seems to have nothing to do with the phantom. The first two cases were in very ordinary gentlemen, and mental influence could not fairly be assigned as any reason for their mimicry of tumours.

There should be no great difficulty of diagnosis in the ordinary cases of this kind. A real tumour that lies deep in or beneath thick muscle may be pressed down beyond clear touch when the muscle contracts and hardens; and it may be difficult to feel the boundaries of a venous tumour or nævus in a deep or thick muscle. But these things will not mislead you if you will study the case

long enough to be clear as to the inconstancy of appearance. If in one minute an apparent tumour is under the fingers and in the next minute is gone; if it shifts from one part of a muscle to another, as one set of fibres after another contracts; if it wholly disappears when the muscle is long relaxed,—this is a phantom, a mere mimicry of a tumour.

There is rather more difficulty in some of the cases of phantom tumours which occur in the abdominal muscles of hysterical women. These are sometimes large, hard, and more nearly fixed in place, size, and form, than those in the limbs, and they may be deceptively complicated with disorderly states of the intestines, or the aorta, or other abdominal or pelvic organs. But in any case, however difficult of diagnosis, ether or chloroform will bring all the help you need. With complete insensibility and muscular relaxation all signs of tumour disappear—the phantom vanishes.

Let me tell you the most striking case of this kind that I have seen. It was in a healthy-looking woman, about thirty years of age, in Sitwell ward. I admitted her for a tumour in the upper part of the abdomen, behind, as it seemed, the first division of the left rectus abdominis muscle, but larger than that seemed. It was roundish, firm, nearly hard, constant in its characters and place, often painful, and distinctly pulsating, like an aneurism. A light bruit was audible in it. Of its history I only remember well that there was nothing to help in diagnosis. In full medical and surgical consultation with my colleagues, the questions were whether there was a tumour with pulsation communicated from the aorta, or an aneurism. If a tumour, where, or in what? If an aneurism, of what artery? The opinions were many and various, partly, perhaps, because the exami-

nation was painful and, therefore, possibly, incomplete. So one day I gave the patient chloroform, and the tumour, the aneurism, and the doubt dispersed; they were all phantoms.

This case, in which an abnormal nervous condition of a muscle and of the abdominal aorta appeared to be combined, may lead us to the next set of mimic tumours—the imitations of aneurisms by what I suppose to be abnormal nervous conditions of arteries. The evidences of this condition are, indeed, not complete; but, at least, the mimicry of aneurism is more frequent in persons of nervous constitutions than in any other. It is sometimes associated with well-marked hysteria, and the likeness of aneurism often varies according to the state of the patient's nervous system.

This mimicry is most frequent in the abdominal aorta, in which it has often been described as a nervous abdominal pulsation; but, so far as I know, it is not described as occurring in the subclavian, innominate, or carotid arteries, though in these it is not very rare, and sometimes is not easy of diagnosis. The chief characteristic of the mimicry is that the affected artery feels much larger than it should be, and pulsates largely at least in one direction, dilating obtrusively, and often so much more visibly than in an ordinary person that it is hard not to believe that it is largely dilated. And yet there certainly is no considerable dilatation; it is rather as if the arterial walls were thin and had too little muscular resistance, as we might suppose them to be in a condition of partial paralysis of their muscular tissue. In the most marked case of imitation of abdominal aneurism that I have seen, examination after death found no disease. A lady of very nervous constitution had suffered severe sea-sickness in a passage to Ireland. The sickness scarcely ceased

during her stay there, and was aggravated on her return-voyage, after which she continued vomiting nearly all her food, till, at the end of some weeks, she died exhausted. During these weeks there was so large and strong a pulsation in her abdominal aorta that some were convinced that she had abdominal aneurism. As she lay on her back one could see the artery pulsating behind the abdominal wall. It raised the hand laid on it, thrusting up the fingers with a firm throb and a thrill, and a low but distinct blowing was audible when the stethoscope was lightly pressed on the artery. Yet there was no aneurism. After death the artery was found absolutely healthy in both size and structure.

I say again, I do not know what is exactly the state of the arteries in these cases. There are few opportunities of examining them after death ; and I have heard of none that told more than the one I just now mentioned to you. But, clinically, these cases are well marked. The artery feels large, wide, and full ; but soft and compressible, or even, if one may so call it, puffy, without any of the hardness or stiffness felt in aneurisms. The pulsation is full, but rather soft, like that of an artery in the reaction after large loss of blood ; and, (which is chiefly characteristic of the absence of dilatation or aneurism,) the extra-full pulsation is in only one direction. There is no unusual lateral dilatation ; the too-much throb is only forwards ; and, if the patient sits upright or leans forward, the pulsation may seem to cease.

The likeness to aneurism in these cases is sometimes greater than could well be expected ; for many things besides the excess of pulsation may contribute to it. In the abdominal aorta an unusual appearance of prominence may be given by an unusual anterior curve of the lumbar vertebræ ; and by thinness, with concavity of the abdo-

minal walls, and emptiness of the stomach. Hardness or toughness of the pancreas, or of lymph-glands about the aorta, may yet further increase the likeness to abdominal aneurism.

In like manner, the imitation of subclavian aneurism is made more marked when a nervously pulsating subclavian artery has glands beneath or about it; and still more when it lies on a cervical rib, and is somewhat raised and, I suppose, widened. And the imitation of upper carotid aneurism is added-to by the very bulbous form of the first portion of the internal carotid and its occasionally tortuous course. That of the lower carotid aneurism may be augmented by enlargement of an overlying lobe of the thyroid gland.

With all these helps to difficulty you may sometimes be troubled in the diagnosis of a mere nervously pulsating artery. In nearly all the cases I have had to see aneurism was suspected; but a few rules will ensure against error. The nervous artery has no lateral expansion; it does not open the finger and thumb when they laterally compress it lightly; you can trace the straight lines of its sides; in the abdominal aorta the pulsation is lost, or nearly lost, when the patient sits leaning forward; in the other arteries, by relaxing the parts over them. Always the artery has at least its natural softness and compressibility; commonly it has more. There are no paroxysms of pain; and if the case has been watched for months, or even years, there is little, or more often no, increase of size. On the other hand, the extent of artery which may feel dilated may much increase, so that we may see and feel, for instance, an abnormal throbbing in the innominate, carotids, and subclavians of the same patient—a state unheard-of in aneurisms.

Before leaving this subject, I just mention to you the

cases in which you will find, in certain patients, anæmia, enlarged thyroid gland, protruding eyeballs, and pulsating carotid arteries. The study of these cases might clear the obscure pathology of the mimic aneurisms; but with this I am not occupied, so I leave them, and go-on to the last set of tumours that I have to speak of.

It may seem absurd to say that cancerous tumours can ever be imitated by any nervous disorder; and, indeed, they cannot; yet you will find few cases requiring a more positive diagnosis than those in which you will have to assert, as beyond all doubt, that a patient has not cancer, but neuralgia.

The cases in which the diagnosis is most often necessary are those of suspected cancers of the breast and of the tongue. Of course any part may be similarly suspected; but, taking all other parts together, they would not supply, at least in surgical practice, so large a number of cases of suspicion as these two.

For the breast, the usual case is that a patient complains of all the pains commonly described as characteristic of cancer. She has dull, aching pain, it may be, of the whole or some part of the breast, and often the pain extends down the arm, more often goes right through to the scapula; and sometimes it is a darting, lancinating pain, shooting this way or that, or burning. I think that you will not find any pain of cancer of the breast which will not be described by patients with this neuralgic mimicry of the disease.

But, however severe the pain, and however like what is supposed to be characteristic, your diagnosis may be clear. That is not cancer which you cannot feel. Cancer is a growth, not any kind of pain, and indeed in its early stages seldom attended with pain; so that when pain is strongly marked, and it is very difficult, or impossible, to

feel any tumour or 'lump' in the breast, you may be certain that there is no cancer.

In most of these cases the pain has an entirely mental origin. The patients have been seeing or hearing of cancer, and their minds have been filled with thoughts of the pain and misery of the disease, till the idea has generated sensations like those from which it was derived.

I have said 'the mind has been filled'; but this is not necessary. I have known these pains mimicking the pains of cancer in persons who had indeed often thought of cancers, but never intently, or with any sustained fear. Just as one may, in dreaming, have vivid impressions of objects he has rarely, perhaps only once, seen, and scarcely ever thought of, so may these imitative pains be felt keenly and often in those who have seldom considered or feared them.

In these wholly mental cases the confident assertion of your judgment will commonly suffice for cure. Sometimes, indeed, patients need repetition of the assertion, especially those who are hysterical. Their trust seems to die-out after a few weeks or months, and some of them rather prefer the disease, or the pity to which it would give them a claim.

There are cases, however, in which the pain is not wholly mental. Neuralgia of the breast sometimes follows a blow. That which in ordinary persons would be followed by aching for some inconsiderable time is apt to be followed in the nervous by long-abiding pain, which the mind will invest with all the characters of what it supposes to be the pain of cancer. And, again, neuralgia of the breast is not rare in gouty or otherwise 'painful' persons, and most of these are ready to fear that their pain is due to cancer, and to endow it with the characters supposed to be distinctive. As fear can give to

any harmless form the shape and substance of that which it most dreads, and can see spectral terrors in a scarecrow, so can it give to ordinary neuralgic pains any of the characters of the pains of cancer. It is especially prone to do this when the neuralgia is felt in a part notoriously liable to cancer. In this part fear will make pain cancerous, even while neuralgic pains, quite as severe, are common in other parts of the body, and in them are regarded without dread.

The diagnosis is more difficult when, with the pain described as like that of cancer, there is, or appears to be, some change of structure of the breast ; for a very nervous patient may endow any mammary disease with any form of pain.

In these cases—which, however, are far more rare than those of neuralgia without change of structure—you must make your diagnosis on the characters of the tumour itself, if there be one. But be cautious about one or two fallacies. Many neuralgic breasts are a little larger than their painless fellows. But mere largeness is not tumour, much less cancer. And many mammary glands, whether neuralgic or not, are rather firmer or tougher in one part than in others ; and when the tough part is grasped, and, as it were, doubled between the thumb and fingers, it may feel something like a hard tumour. Let me advise you not to use this mode of examining a mammary gland. I have known it often fallacious, never useful. If you cannot feel a tumour by feeling or pressing over and round it, you may believe there is not one ; you may be certain there is not a hard cancer.

And if you can feel a tumour, do not believe it to be cancerous merely because it is painful. Judge by what you can feel, not by what the patient feels ; remembering always that in the early stages of cancer of the breast, in

which alone there is any difficulty of discerning it by its tangible characters, it is rarely very painful. All the popular ideas of the pains of cancer are derived from the cases of advanced disease, or of cancers growing rapidly or inflamed or ulcerating. In early cases, and in nearly all in which the diagnosis is obscure, the greater the pain the less is the probability of cancer. And, let me add, neuralgia, even of the severest kind and longest duration, does not tend to cancer of the breast. I must have seen more than a hundred cases of neuralgia, and I have seen only one in which it was followed by cancer, and I have no belief that in this one the sequence was a consequence.

Most of what I have said of the breast might be applied, with change of name, to the mimicry of cancer of the tongue. Nervous people hear of the cancer, and then, with less or more of thought about it, have pain and aching in their own tongues, usually at the side and in that frequent seat of cancer, at the junction of the middle and posterior thirds. Others have neuralgia of the tongue; a dull, heavy aching on one side of it—a condition which is far from rare in the habitually neuralgic, especially, I think, in the gouty—and these give to their neuralgic pains what they believe to be cancerous characters. And others, with dyspepsia, have sore tips or edges of their tongues, which they, too, magnify.

It is strange to observe the tenacity with which some of these patients cling to the most dismal view of their cases. Though nothing wrong can be seen or felt, and though months, or even (as I have known) years, may pass without any disease appearing, yet will they believe themselves on the brink of misery with cancer of the tongue. But your diagnosis may be as fixed as their fears and had better be more positive. That is not cancer which you can neither see nor feel. And do not be

deceived by an unusually large cluster of papillæ of the circumvallate group, or by any little thickening due to irritation of a tooth, or by a fissure or psoriasis. A patient's fear will give any of these the sensations of cancer; but, as I said of the breast, so here, your diagnosis must be founded, not on the patient's sensations, but on your own. As observers from without, you must insist that you are better than the observer from within.

It is scarcely possible to write usefully on the treatment of a group of diseases such as those to the diagnosis of which these lectures are devoted. The central fact in every case, that is, the nervous mimicry dependent on a nervous constitution, may be constant; but everything around it may be, in each case, different. For the central condition there is no direct remedy; for some of the conditions around it there may be.

In each case it is well to consider that treatment has to be directed against at least three different things: the local symptoms, the constitutional condition which may co-exist or be combined with the nervous, and the nervous constitution itself. Each of the first two may be treated as it would be in other than nervous patients, provided the treatment will not do damage to the general condition of the nervous system.

1. Thus, for pain, general narcotics do more general harm than local good; they should not be used except under urgent necessity. A nervous patient who habitually or frequently takes any narcotic is usually, I think, past hope of remedy by anything but long time. Local soothing means should be always used, and are sometimes very beneficial, such as frictions with solutions of opium, aconite, or belladonna, or plasters with belladonna or opium. All such frictions should be preceded by spongings of the part

with very hot water, so that the skin at the time of friction may be, as one may say, red-hot. And this hot sponging is, of itself, often very useful. Indeed, I have known nothing so good for the painful aching spines which are common in the mimics of disease, as that the patient should sit in tepid water, and have the spine drenched again and again with water as hot as can be borne, and then warmly covered and rested. Such drenching with water wrung from sponges or cloths is better than the more forcible hot douches; they are apt to shock too much. Similar uses of hot water and of very hot poultices are advisable also for joints and other parts imitating pains of real disease; and I believe it would be hard to find a case in which cold applications, whether lotions, douches, icings, or any other, are not mischievous. If I were to make an exception, it would be, doubtfully, for the occasional use of the ether-spray-freezing in an intense neuralgic mimicry.

For parts that are nervously, or by erroneous long rest, stiffened, contracted, or distorted, forcible movements may be used, as I have said in the last lecture, and in that on bone-setting. Besides, speaking generally, such parts should be regularly exercised, up to any degree short of that which repeatedly produces exhausting pain or fatigue, or is not recovered-from after a night's rest. However little can be done without these or the like consequences, it should be done, though it be but the raising of a finger ten times a day. Generally, too, the use of nervously affected parts should always be for short periods, and frequent in the day; and after every time of exercise there should follow a much longer rest.

And this rule of long rest after exercise should be observed with the use of frictions, shampoos, douches, and other like things. Rest should follow them all.

Among other local means of treatment I think I may certainly say that local bleeding is always useless or worse ; that severe counter-irritation, or any such as produces general feverishness, usually does harm, never does real good ; that light and transient counter-irritation often seems useful, and may be very often repeated.

Galvanism is, I do not doubt, in many cases useful. Certainly, it is so in the cases of muscular inaction, whether the will-less or the power-less. But in the haphazard way in which it is commonly employed it is sometimes mischievous, and often useless, unless by its mental influence. Its real value is not likely to be gained without a much more careful study than is usual.

2. I believe that any other constitutional condition may co-exist or be combined with the nervous, and may make not only the diagnosis but the treatment the more difficult. Whatever treatment is employed must have respect to each constitution. The scrofulous, the tuberculous, the gouty, the rheumatic constitution may need treatment as much as the nervous. It is through this great variety of conditions complicating the nervous disorder that so great variety of treatments are supposed useful. Many patients are anæmic, and iron does them good ; some are tuberculous, and some scrofulous, and these need iodides, or cod-liver oil, or various tonics ; some have lithic acid diathesis ; some oxalate, and the remedies that are useful to these may seem mischievous to others. A single principle of treatment governs what may be said of all these and other complications : any error, whether local or general, which complicates the essential disorder in the nervous system should, if possible, be remedied ; for every such error is to the nervous system as an irritant or disturbance augmenting its disorder. Under this principle it is that in some of the

cases of nervous mimicry the treatment of some malady of the uterus has seemed to cure the whole trouble ; and in some, constant purgation ; in some, zinc ; in some, arsenic or phosphorus. I know that in many cases all these things are useless. I believe that when they have done good, it has been by the cure of something complicating the nervous disorder, which being removed left the nervous system able to recover of itself.

3. Then, lastly, for the treatment of the nervous system itself. There is no medicinal remedy, nor, I think, any medicine which can be said to be generally useful. Iron and zinc and various tonics often seem to do some good for a time, even in those in whom there is no evidence of such complication as I just spoke of ; but they do not cure.

That which I think has always to be cared-for is the full nutrition of the nervous system, by means of right food, sleep, rest, warmth and other common things of life.

The food should be ample, nutritious, mixed. No cases seem to me harder to remedy than those of nervous patients who say they cannot eat. With many of them it is only that they cannot will to eat ; with some that they have so long not willed to eat, that at last the desire and all sensation of hunger have ceased. But till they do eat, I believe that nothing will do good.

If there be cases worse than these, they are the cases of the patients who cannot eat but will drink. Those who reject all food, and yet drink stimulants often in the day—stimulants of whatever kind—have no chance of being cured. They may outlive their malady, but this is the best that can be hoped for.

As to what may be eaten or drunk, I believe that very few general rules can be stated. Large quantities of tea, and of coffee, and of sugar are, I believe, generally

injurious to nervous patients, and much of strong tobacco-smoking ; but for the rest, if any selection of foods needs to be made, it must be for other errors or defects of health more than for those of the nervous system.

Sleep and rest should be carefully arranged. As a rule, I think that nervous patients should sleep at least eight hours in the twenty-four, and they may sleep one or more in the day as well as at night. But I am disposed to think that even sleeplessness is less mischievous than the frequent or habitual use of narcotics. And whether with or without sleep all exercise should be followed by long-continued rest.

Warmth is always advisable—warmth of air and clothing and bedding. In some cases very fresh mountain air seems to have its full invigorating effect ; but I think these are only among such as can be active ; others must be kept warm.

But, perhaps, the most important part of the treatment of these cases is the mental part. I have referred to the infrequency of commonplace minds among the patients with nervous mimicry—some being far above, some far below, some in various ways divergent from, the ideal standard average. It would, probably, always tend to the remedy of nervous mimicry if the mind could be brought to an average and uniform level, to a just medium of common sensibility and common sense. A few excellent and wise persons might be the worse for such a change ; but for all except these the change would be for the better and a chief step towards recovery.

Most of all, the will needs education in these cases. It needs to be trained to the cure of the mimicry, to the endurance of pain, to the control of movements, to the fixing of the attention on anything rather than the supposed disease. And very often, in the worst cases,

this training of the will is not possible unless the patient be separated from the persons and things associated with the disease. Many patients cannot get well at home. Some of those about them are too sympathetic ; some too hard ; some yield too much or too soon : none are really helpful ; and the patient's will becomes constantly more feeble, or more widely perverted. In conditions such as these the patient should live with quiet sensible strangers, who can teach the will and exercise and control it.

The effect of judicious education of the will in the worst cases of nervous mimicry is sometimes very striking ; complete recovery is not rare, especially in cases of mimic loss of power in the spine and limbs, and of mimic diseases of joints, and mimic gastric disorder and apepsia. But the teacher must be carefully chosen ; for among these nervous patients are some who are ready to become the very slaves of persons who have strong wills, or who profess that they are possessed of knowledge or authority that cannot or may not safely be resisted. Thus it is that the worst cases are sometimes cured by the most ignorant persons, who, by the mere confidence of their assertions, give confidence and will : but the consequences of such cures may be as bad as the disease.

VIII.

ON DISEASE OF THE MAMMARY AREOLA PRECEDING CANCER
OF THE MAMMARY GLAND.*Published in the St. Bartholomew's Hospital Reports for 1874.*

I BELIEVE it has not yet been published that certain chronic affections of the skin of the nipple and areola are very often succeeded by the formation of scirrhus cancer in the mammary gland. I have seen about fifteen cases in which this has happened, and the events were in all of them so similar that one description may suffice.

The patients were all women, various in age from 40 to 60 or more years, having in common nothing remarkable but their disease. In all of them the disease began as an eruption on the nipple and areola. In the majority it had the appearance of a florid, intensely red, raw surface, very finely granular, as if nearly the whole thickness of the epidermis were removed; like the surface of very acute diffuse eczema, or like that of an acute balanitis. From such a surface, on the whole or greater part of the nipple and areola, there was always copious, clear, yellowish, viscid exudation. The sensations were commonly tingling, itching, and burning, but the malady was never attended by disturbance of the general health. I have not seen this form of eruption extend beyond the areola, and only once have seen it pass into a deeper ulceration of the skin after the manner of a rodent ulcer.

In some of the cases the eruption has presented the

characters of an ordinary chronic eczema, with minute vesications, succeeded by soft, moist, yellowish scabs or scales, and constant viscid exudation. In some it has been like psoriasis, dry, with a few white scales slowly desquamating; and in both these forms, especially in the psoriasis, I have seen the eruption spreading far beyond the areola in widening circles, or, with scattered blotches of redness, covering nearly the whole breast.

I am not aware that in any of the cases which I have seen the eruption was different from what may be described as long-persistent eczema, or psoriasis, or by some other name, in treatises on diseases of the skin; and I believe that such cases sometimes occur on the breast, and after many months' duration are cured, or pass by, and are not followed by any other disease. But it has happened that in every case which I have been able to watch, cancer of the mammary gland has followed within at the most two years, and usually within one year. The eruption has resisted all the treatment, both local and general, that has been used, and has continued even after the affected part of the skin has been involved in the cancerous disease.

The formation of cancer has not in any case taken place first in the diseased part of the skin. It has always been in the substance of the mammary gland, beneath or not far from the diseased skin, and always with a clear interval of apparently healthy tissue.

In the cancers themselves, I have seen in these cases nothing peculiar. They have been various in form; some acute, some chronic, the majority following an average course, and all tending to the same end; recurring if removed, affecting lymph-glands and distant parts, showing nothing which might not be written in the ordinary history of cancer of the breast.

The single noteworthy fact found in all these cases is that which I have stated in the first sentence, and I think it deserves careful study. For the sequence of cancer after the chronic skin-disease is so frequent that it may be suspected of being a consequence, and must be always feared, and may be sometimes almost certainly foretold. I believe that a nearly similar sequence of events may be observed in other parts. I have seen a persistent 'rawness' of the glans penis, like a long-enduring balanitis, followed after more than a year's duration by cancer of the substance of the glans. A chronic soreness or irritation (of whatever kind) on the surface of the lower lip often long precedes cancer in its substance; and, with a frequency surpassing all other cases of the kind, the superficial syphilitic diseases of the tongue are followed, and not superseded, by cancers which do not always appear to commence in a diseased part of the tongue.

For an explanation of these cases it may be suggested that a superficial disease induces in the structures beneath it, in the course of many months, such degeneracy as makes them apt to become the seats of cancer; and that this is chiefly likely to be observed in the cases of those structures which appear to be, naturally, most liable to cancer, as the mammary gland, the tongue, and the lower lip. One may suspect that similar surface-irritation has much to do with the frequency of cancer of the rectum, pylorus, and ileo-cæcal valve, in any of which parts the degeneracy, which might come naturally in old age and make them apt for cancer, may be hastened, and made prematurely sufficient, by an adjacent disturbance of nutrition.

In practice, the question must be sometimes raised whether a part through whose disease or degeneracy cancer is very likely to be induced should not be removed.

In the member of a family in which cancer has frequently occurred, and who is at or beyond middle age, the risk is certainly very great that such an eruption on the areola as I have described will be followed within a year or two by cancer of the breast. Should not, then, the whole diseased portion of skin be destroyed or removed as soon as it appears incurable by milder means? I have had this done in two cases, but, I think, too late. Or, again, when one with a marked family-liability to cancer has syphilitic disease of the mucous membrane of the tongue, with frequent recurrences of inflammation, should not all the worst pieces of the membrane be removed? I should certainly advise it, especially if the membrane were ichthyotic, if it were not that the disease is commonly so extensive that good scar-tissue would not be likely to be formed, and that bad scar-tissue, often irritable and ulcerating, is as likely to induce cancer as the syphilitic or ichthyotic patches would have been.

IX.

ON A FORM OF CHRONIC INFLAMMATION OF BONES
(OSTEITIS DEFORMANS).

Read before the Royal Medical and Chirurgical Society of London, November 14, 1876 ; published, with five plates, in the Society's Transactions, 1877.

I HOPE it will be agreeable to the Society if I make known some of the results of a study of a rare disease of bones.

The patient on whom I was able to study it was a gentleman of good family, whose parents and grandparents lived to old age with apparently sound health, and among whose relatives no disease was known to have prevailed. Especially, gout and rheumatism, I was told, were not known among them ; but one of his sisters died with chronic cancer of the breast.

Till 1854, when he was forty-six years old, the patient had no sign of disease, either general or local. He was a tall, thin, well-formed man, father of healthy children, very active in both mind and body. He lived very temperately, could digest, as he said, anything, and slept always soundly.

At forty-six, from no assigned cause, unless it were that he lived in a rather cold and damp place in the North of England, he began to be subject to aching pains in his thighs and legs. They were felt chiefly after active exercise, but were never severe ; yet the limbs became less agile or, as he called them, 'less serviceable,' and

after about a year he noticed that his left shin was mis-shapen. His general health was, however, quite unaffected.

I first saw this gentleman in 1856, when these things had been observed for about two years. Except that he was very grey and looked rather old for his age, he might have been considered as in perfect health. He walked with full strength and power, but somewhat stiffly. His left tibia, especially in its lower half, was broad, and felt nodular and uneven, as if not only itself but its periosteum and the integuments over it were thickened. In a much less degree similar changes could be felt in the lower half of the left femur. This limb was occasionally but never severely painful, and there was no tenderness on pressure. Every function appeared well discharged, except that the urine showed rather frequent deposits of lithates. Regarding the case as one of chronic periostitis, I advised iodide of potassium and *Liquor Potassæ*; but they did no good.

Three years later I saw the patient with Mr. Stanley. He was in the same good general health, but the left tibia had become larger, and had a well-marked anterior curve, as if lengthened while its ends were held in place by their attachments to the unchanged fibula. The left femur also was now distinctly enlarged, and felt tuberos at the junction of its upper and middle thirds, and was arched forwards and outwards, so that he could not bring the left knee into contact with the right. There was also some appearance of widening of the left side of the pelvis, the nates on this side being flattened and lowered, and the great trochanter projecting nearly half an inch further from the middle line. The left limb was about a quarter of an inch shorter than the right. The patient believed that the right side of his skull was enlarged, for

his hats had become too tight ; but the change was not clearly visible.

Notwithstanding these progressive changes, the patient suffered very little ; he had lived actively, walking, riding, and engaging in all the usual pursuits of a country gentleman, and, except that his limb was clumsy, he might have been indifferent to it. He had taken various medicines, but none had done any good, and iodine, in whatever form, had always done harm.

In the next seventeen years of his life I rarely saw him, but the story of his disease, of which I often heard, may be briefly told and with few dates, for its progress was nearly uniform and very slow. The left femur and tibia became larger, heavier, and somewhat more curved. Very slowly those of the right limb followed the same course, till they gained very nearly the same size and shape. The limbs thus became nearly symmetrical in their deformity, the curving of the left being only a little more outward than that of the right. At the same time, or later, the knees became gradually bent, and, as if by rigidity of their fibrous tissues, lost much of their natural range and movement.

The skull became gradually larger, so that nearly every year, for many years, his hat, and the helmet that he wore as a member of a Yeomanry Corps, needed to be enlarged. In 1844 he wore a shako measuring twenty-two and a half inches inside ; in 1876 his hat measured twenty-seven and a quarter inches inside. In its enlargement, however, the head retained its natural shape and, to the last, looked intellectual, though with some exaggeration.

The changes of shape and size in both the limbs and the head were arrested, or increased only imperceptibly, in the last three or four years of life.

The spine very slowly became curved and almost rigid. The whole of the cervical vertebræ and the upper dorsal formed a strong posterior, not angular, curve; and an anterior curve, of similar shape, was formed by the lower dorsal and lumbar vertebræ. The length of the spine thus seemed lessened, and from a height of six feet one inch he sank to about five feet nine inches. At the same time the chest became contracted, narrow, flattened laterally, deep from before backwards, and the movements of the ribs and of the spine were lessened. There was no complete rigidity, as if by union of bones, but all the movements were very restrained, as if by shortening and rigidity of the fibrous connections of the vertebræ and ribs.

The shape and habitual posture of the patient were thus made strange and peculiar. His head was advanced and lowered, so that the neck was very short, and the chin, when he held his head at ease, was more than an inch lower than the top of the sternum.

The short narrow chest suddenly widened into a much shorter and broad abdomen, and the pelvis was wide and low. The arms appeared unnaturally long, and, though the shoulders were very high, the hands hung low down by the thighs and in front of them. Altogether, the attitude in standing looked simian, strangely in contrast with the large head and handsome features.¹

All the changes of shape and attitude are well shown in sketches from photographs taken six months before death. Only the lowering of the necks of the femora is not shown. In measurement after death the axes of the

¹ An attitude somewhat similar is given by a rare form of what I suppose to be general chronic rheumatic arthritis of the spine involving its articulations with the ribs. The spine droops and is stiff, the chest is narrow, the ribs scarcely move, the abdomen is low and broad, but there is no deformity of head or limbs.

shaft and neck of the right femur formed an angle of only 100° instead of 120° or 125° , and this change of shape added to the appearance of increased width of the pelvis.

But with all these changes in shape and mobility of the head, spine, and lower limbs, the upper limbs remained perfect, and there was no disturbance of the general health.

In 1870, when the disease had existed sixteen years, the left knee-joint was, for a time, actively inflamed and its cavity was distended with fluid. But the inflammation soon subsided, only leaving the joint stiffer and more bent.

About this time some signs of insufficiency of the mitral valve were observed, but the patient now lived so quietly, and moved with so little speed, that this defect gave him no considerable distress.

In December, 1872, sight was partially destroyed by retinal hæmorrhage, first in one eye, then in the other,¹ and at nearly the same time he began to be somewhat deaf. In the summer of 1874 he had frequent cramps in the legs, and neuralgic pains, which were described as 'jumping over all the upper part of the body except the head,' but change of air seemed to cure them.

In January, 1876, he began to complain of pain in his left forearm and elbow which, at first, was thought to be neuralgic. But it grew worse, and swelling appeared about the upper third of the radius and increased rapidly, so that, when I saw him in the middle of February, it seemed certain that a firm medullary or osteoid cancerous growth was forming round the radius.

¹ Mr. Brudenell Carter saw him in January, 1873, and observed 'the right retina sprinkled with small dots of arterial hæmorrhage, chiefly in parts remote from the centre'; and 'there was no other change.' The left retina was at this time healthy, but in February Dr. Clifford Allbutt found 'several little plugs' in its vessels.

Still the general health was good. Auscultation could detect mitral disease, but the appetite and digestion were unimpaired, the urine was healthy, the mind as clear, patient, and calm as ever. As letters about him at this time said, 'his general health has been excellent'; 'he is free from pain except in the left arm; he sleeps well, enjoys himself, and does not know what a headache is.'

After this time, however, together with rapid increase of the growth upon the radius, there were gradual failure of strength and emaciation, and on the 24th of March, after two days of distress with pleural effusion on the right side, he died.

The body was examined five days after death, and showed no marked signs of decomposition. As it lay on a flat board its posture was remarkable, for the head was upraised to the level of the sternum, being supported by the rigid and arched spine, and the lower limbs, with the knees bent and stiff, rested on the heels and nates.

The pericranium, dura mater and all the substance of the brain appeared healthy.

The right pleural cavity contained at least a pint of pale serous fluid, with flakes and strings of inflammatory exudation. The lung was compressed, and in its pleural covering were numerous small nodular masses of pale cancerous substance. The proper pulmonary structure appeared healthy, and so did the left lung and its pleura, except that in the pleura and anterior mediastinum there were many small masses of cancer.

The heart was enlarged but thin-walled. The tricuspid and pulmonary valves and artery were healthy; the mitral valve was opaque, contracted, stiffened with atheromatous and calcareous deposits.

The aortic valves were slightly opaque but pliant, and

both in them and in the first part of the aorta were numerous small patches of atheroma.

The liver and digestive canal and kidneys, examined externally, appeared healthy.

The right femur, the left tibia, the patellæ, and the upper part of the skull, were taken for separate examination, and will be separately described.

In the other bones of the skeleton, except the left radius, no signs of disease appeared externally, but I regret that they were not all more carefully examined, for I think that, at least in the clavicles and pelvis, some changes like those in the long bones of the lower limbs would have been found.

The upper third of the left radius was involved in a large ovoid mass of pale grey and white soft cancerous substance, similar to that of the nodules in the pleuræ and mediastinum, but with growths of bone extending into it. The rest of the radius and the ulna appeared quite healthy.

Some nodules of similar cancerous substance were imbedded in the bones of the vault of the skull.

The curvatures of the spine and its rigidity appeared due to shortening and hardening of its fibrous structures. The vertebræ appeared healthy; there was no appearance of overgrowth or anchylosis among them.

In no part, whether near or far from the diseased bones, was there an indication of any change of structure in skin, muscle, tendon, or fascia; but in the right hip-joint and in the left knee-joint there was some thinning and wasting of articular cartilage, such as one sees in chronic rheumatic arthritis. The other hip- and knee-joints and both ankle-joints were healthy.

In the arteries of the lower limbs there was extensive atheromatous and calcareous degeneration.

The enlargement of the skull may be estimated by comparison of the following measurements:

	Diseased skull.	Average skull.
Circumference at the level of the middle of the tem- poral fossa	26½ in.	21 in.
From occipital spine to base of nasal bones	15 in.	13¼ in.
From mastoid to mastoid process	18½ in.	15½ in.

All the sutures, at least all those of the vertex, were obliterated. The outer surface of the upper part of the skull was lowly bossed by the predominant thickening of the hinder part of the parietal bones. The thickness was in every part increased to the extent shown in these following measurements:

		Lines.
In a median vertical section the thickness of the frontal bone was		11—13
" " parietal .		14—16
" " occipital .		8—12
In a horizontal section, through the middle of the temporal fossa,		
the thickness of the frontal bone was		8—9
" temporal „		6—9
" at their junction		2
" of the occipital bone was		10—12

Comparing these measurements with those of average healthy skulls, it may be said that the bones of the vault of this skull were in every part increased to about four times the normal thickness.

The whole outer surface of the skull-cap was finely porous; in the least changed parts, such as the squamous bone, perforated with innumerable apertures for blood-vessels; in the most changed, finely reticulate, as with delicate cancellous and medullary texture.

The inner surface was comparatively smooth and appeared little changed, except by the enlargement of all channels and apertures for blood-vessels, and especially by the deepening of all the grooves for the middle meningeal artery and its branches.

On the cut surface, in the median vertical section, that which might be regarded as the altered internal table of the skull was a layer, having a very unequal thickness varying from two to six lines, consisting of hard white bone, close-textured, in some parts porous or finely reticulate, in more looking compact and dense like limestone or white brick.

The rest of the thickness of this part of the skull, representing probably the altered diploë and outer table, was made up of bone in various degrees porous, cancellous, or cavernous, with spaces filled with soft reddish substance, a kind of medulla. Its surface was covered with a very thin layer, a mere coating of more finely porous bone.

In the horizontal section, at the level of the upper part of the squamous bone, the same altered characters were observable, but a larger proportion of the substance of the skull was finely porous or reticulate.

By the cavities in the skull-cap in which cancerous growths were lodged the structure of the bone was neither more nor less altered than in other parts.

A portion of sphenoid bone showed changes of structure very similar to those already described, but with a much more uniform and regular finely porous condition.

The bones of the face were not uncovered, but they showed, neither to sight nor touch, any appearance of disease ; not a feature was unnatural.

The conditions of all the long bones were so similar that one description may serve for the altered structure of both femora and tibiae.¹

The periosteum was not visibly changed, not thicker or more than usually adherent.

¹ The specimens are in the Museums of the Royal College of Surgeons and of St. Bartholomew's Hospital.

The outer surface of the walls of the bones was irregularly and finely nodular, as with external deposits or outgrowths of bone, deeply grooved with channels for the larger periosteal blood-vessels, finely but visibly perforated in every part for transmission of the enlarged small vessels. Everything seemed to indicate a greatly increased quantity of blood in the vessels of the bone.¹

The medullary structures appeared to the naked eye as little changed as the periosteum. The medullary spaces were filled with soft, yellow, ruddy, and bright crimson medulla, of apparently healthy consistence. The medullary laminæ and cancelli had a normal aspect and arrangement, and in the shafts of the long bones the medullary spaces were not encroached upon.

The compact substance of the bones was, in every part, increased in thickness. Taking, for example, the femur, the thicknesses of its walls and those of a healthy femur of about the same length and age are compared in the following tables.

	Healthy.	Diseased.
	Lines.	Lines.
Thickest parts of the wall . . .	3—6	6—10
Articular covering of head, about . . .	$\frac{1}{4}$	3—10
Wall of neck, about . . .	$\frac{1}{4}$ —3	4—6
Wall of the trochanter major, about . . .	$\frac{1}{4}$ — $\frac{1}{2}$	3—5
Articular covering of the condyles, about . . .	$\frac{1}{4}$ — $\frac{1}{3}$	3—5
Lateral walls of the condyles . . .	$\frac{1}{8}$ — $\frac{1}{4}$	2 and more.

Changes in similar proportions were found in the walls of the tibia. In the patellæ the walls were from three to five lines thick.

The thickening of the walls of the shafts of the bones appeared due chiefly to outward expansion and some superficial outgrowth. In some places there were faint appearances of separation of parts of the outer layers of

¹ But see the account of the microscopic examination.

the walls, and of these becoming thick and porous, while the corresponding parts of the inner layers were less changed; but in the greater part of the walls the whole construction of the bone was altered into a hard, porous or finely reticulate substance, like very fine coral. In some places, especially in the walls of the femur, there were small, ill-defined patches of pale, dense, and hard bone looking as solid as brick.

In the compact covering of the articular ends of the long bones, and in those of the neck and great trochanter of the femur, and in the patellæ, the increase of thickness was due to encroachment on the cancellous texture, as if by filling of its spaces with compact porous, new-formed bone.

Mr. Butlin was so good as to make careful microscopic examination of the diseased bones, and to give me the following report on them, together with drawings of their minute structure.

‘Microscopical examination was made of sections cut from the skull and from the tibia, some of them from the recent bones, but the majority of them from portions of bone deprived of earthy salts and rendered sufficiently soft to be cut with a razor. The appearances observed were essentially the same in both bones, but most of the drawings and description were taken from the tibia, the sections of which were much clearer than those of the skull.

‘The examination was conducted from a twofold point of view: first, to discover the changes which the bone had undergone; second, to discover, if possible, the nature of the process which had led to such changes.

‘With a low power the number of Haversian systems and canals in any given section was seen to be much

diminished. The space between the Haversian canals was occupied by ordinary bone-substance, containing numerous lacunæ and canaliculi. The Haversian canals were enormously widened, many of them were confluent, and thus the appearance of a number of communicating medullary spaces was obtained, an appearance which was rendered still more striking by the presence in the canals of a large quantity of ill-developed tissue in addition to the blood-vessels. With a high power the contents of the Haversian canals were seen to consist generally of a homogeneous or granular basis, containing cells of round or oval form about the size and having much the appearance of leucocytes. Larger nucleated cells were also present, and fibres or fibro-cells, sometimes in considerable quantity. Myeloid cells were occasionally observed, but they were not plentiful; fat also existed in many of the larger spaces, especially in the skull. The vessels were usually small compared with the channels in which they ran; indeed, they did not seem to be much larger than those of normal bone. The walls of some of the canals were lined by a single layer of osteoblasts, a condition precisely similar to that observed in the normal ossification of bone in membrane. The presence of new bone was most evident in the periosteum of the tibia, external to the ordinary compact layer of the shaft. This external layer was, of course, but thin, and was much softer and less developed than the cortex of the bone from which it sprung; it evidently was not nearly sufficient to account for the great increase in the diameter of the tibia. From the diminution in size of the medullary canal it was thought that a similar recent formation of bone would be found on its outskirts, but this expectation was not justified by observation.

‘ With a medium power the number of lamellæ

surrounding the Haversian canals was easily seen to be not larger than in normal bone, whilst the arrangement of the intervening space was most complex and totally different from that of healthy bone. The lacunæ and canaliculi throughout the sections did not strikingly differ from those of ordinary bone.'

I am indebted to Dr. Russell for the following chemical analysis of portions of the diseased skull and tibia, and of a healthy tibia in comparison with them.

	Skull.	Tibia.	Normal tibia.
Inorganic constituents (Ash)	60.59	61.22	63.62
Organic	39.41	38.78	36.38
Phosphoric acid (P_2O_5)	22.76	25.45	25.50
Carbonic „ (CO_2)	3.59	3.95	3.59
Fat	6.83	3.45	—
Moisture in the sample (dried at $115^\circ C.$)	15.49	11.83	9.73
The CO_2 calculated as calcium carbonate ($CaCO_3$)	8.17	8.99	8.16
The P_2O_5 calculated as calcium phosphate ($Ca_3P_2O_8$)	49.70	55.56	55.66
Specific gravity.	1.895	1.889	1.886 ¹

Cases of the disease which I have described are so rare that I believe no one has seen a sufficient number of them to enable him to distinguish this disease, either clinically or anatomically, from some which seem like it. Specimens illustrating it are commonly included under a general name of hyperostosis, osteoporosis, senile rachitis, or the like. But I hope that, if I add to the description I have just given some notes of similar cases which I have seen or found on record, the disease may be so distinguished as to deserve in pathology a separate place and name.

CASE 2.—Some ten years ago I saw a gentleman, between 50 and 60, very active, tall, thin, and muscular,

¹ Specific gravity of normal skull 1.990.

a master of hounds. For many years before his death he had curvature of the thighs and legs, exactly like that already described, and stooping of the spine. The changes of the limbs were attended with severe pains, which he used to relieve with hard rubbing, but the general health was unimpaired. In the last years of his life the upper part of his right humerus became very large, and as he was riding and suddenly raised his arm the bone broke near the shoulder. The evidence of large tumour now became clear, and I amputated the arm at the shoulder-joint. The tumour was well marked and very vascular medullary cancer investing and infiltrating the upper part of the humerus. The rest of the humerus was healthy, and the fracture, which was just below its neck, was evidently due to muscular force acting on its structures spoiled by the cancerous growth. He died a few days after the operation, but was not examined after death. The similarity of his case with that which I have described is, I think, certain.

CASE 3.—I saw, with the late Dr. Brinton, a gentleman between 40 and 50 who may be still living. He was a sturdy and quite healthy man; his tibiæ were curved and enlarged exactly like those in the first case and he had similar pains, but there was more thickening of periosteum and an appearance of more external formation of bone. He was treated with iodide of potassium and many other things as for periostitis, but without avail.

CASE 4.—A case is recorded by Dr. Wilks in the Transactions of the Pathological Society,¹ and through the kindness of Sir William Gull, whom the patient

¹ Vol. xx. p. 273, 1869.

occasionally consulted, I am enabled to add some facts to those in Dr. Wilks's report, and to show photographic portraits.

A summary of Dr. Wilks's report is that the patient was 60 when he died. Signs of the disease, beginning with pains like those of rheumatism in the legs, were first observed fourteen years before his death. It was soon found that the tibiæ were enlarged, and in subsequent years the cranium and nearly all the bones of the skeleton underwent similar changes. About a year before death the general health began to suffer from the thorax having become implicated in the disease. Gradually the chest became more contracted and at last quite fixed; the breathing became more difficult until at last the respiratory apparatus altogether stopped.

Sir William Gull's notes tell that the patient consulted him when 56 years old, and said that he first noticed enlargement in the left tibia when he was 45 years old; that he had seven brothers well and strong, and was eldest in the family. He complained chiefly of weakness, inability to make exertion, feeling of nervousness with occasional vertigo, shortness of breath, stiffness in neck, hoarseness and feebleness of voice. His general health was good; he was not much troubled with pain anywhere; but had occasional strange sensations about the head, and much cough. His height, when a young man, was five feet three and a half inches, now four feet eleven and a half inches. The urine was normal and of normal colour. The cranium was enlarged and thickened; the clavicles much thickened, as also the long bones; the phalanges and facial bones and perhaps the lower jaw were not altered. The ribs were thick and immovable, as was also the sternum. There was general dulness over the chest on percussion. The respiration was chiefly diaphragmatic.

Less than a year before the patient's death Sir William Gull recorded that he was breathless, and had occasional attacks of mental confusion in which he remarked that he could not understand the sense of words. His voice was hoarse and feeble, and the hyoid bone seemed thickened. The head had continued to enlarge, and he maintained that he was still losing in height. The neck was fixed, and somewhat forward. All the viscera appeared normal. The urine, repeatedly examined, was always found normal, and of normal colour.

The record of the post-mortem examination by Dr. Goodhart leaves no doubt that the disease in this case was the same as that which I have described, and it may be important that this patient also had cancerous disease. 'A growth . . . corresponding to the growth described as epithelioma of the arachnoid surface of dura mater,' grew from the inner surface of the dura mater, was as large as a chestnut and made a pit in the brain near the left Sylvian fissure.

The description of the changed structure of the bones, for which I may refer to the 'Pathological Transactions,' seems to me to indicate that the disease was more advanced in the direction of degeneracy than that which I have described, or that it had not been in any degree repaired.

CASE 5.—I owe to Mr. Bryant the opportunity of seeing a similar case which was under his care in Guy's Hospital, and of which Mr. Viney was so good as to give me notes.

The patient was a carpenter, 60 years old, a hard-working married man, and had seven children. When about 16 years old he had a slight attack of gonorrhœa, but without sores, and no history of syphilis could

be learned. When 35 years old he received an injury to his pelvis. Shortly after this he had trouble with his bladder, which became much distended; a large quantity of clotted blood was washed out. He lay in bed for this six weeks, and at the end of three months was able to go to work again.

For the last five years he had been troubled with gout in his left great toe. His father suffered from this. The attacks had been short; a few days' rest always sufficed for recovery.

About three years before admission he first felt pains of a shooting description about the tendons of the popliteal space, whenever he straightened his legs. At this time also he first noticed a swelling of the legs, which began at the ankles. These symptoms, without his taking any special notice of them, continued for about a year.

In the last year and a half the tibiæ had become much swollen and curved forwards, and on account of the pain he had in them from standing he had been obliged to give up his regular work. Until admission he did not notice anything wrong with his other bones, but he had lost about half an inch in height.

The tibiæ presented a marked curve forwards. The anterior border of each was rounded to a very marked degree, so that it could not be felt at all distinctly. The right tibia was slightly larger than the left. The inner surface of each measured about four inches at its widest part. The veins above the ankle were in a varicose condition.

The fibulæ were very much enlarged; the femora enlarged in their shafts and bowed outwards. The great trochanter was drawn up to the level of a vertical line drawn from the anterior superior spinous process of the

ilium to the horizontal line of the body, instead of being about two and a half inches below this line. The patellæ were little larger than natural.

The bones of the upper extremity were enlarged, but not to so marked a degree as those of the lower. The enlargement was most marked in the humeri and the left was thicker than the right. He could not straighten his arms, probably owing to the enlargement of the olecranon. In the clavicles the natural curves were very much increased and the bones thickened, the left more so than the right. In the scapulæ the spines and acromion processes were very much enlarged.

The chest was slightly flattened from side to side, but moved fairly whilst breathing. The ribs on the right side were slightly larger than those on the left.

There was a general curve backwards from the cervical to the dorsal vertebræ, so that the patient's usual position in bed was with his head bent forwards, and his legs in a semi-extended position.

The bones of the hands and feet did not seem to have shared in the general thickening.

There seemed to be a slight thickening about the external protuberance of the occipital bone, but there was no other evidence of the cranial bones being involved.

The patient had cold perspirations over his legs in the evening. His urine had a specific gravity of 1014, was strongly acid, contained a little albumen but no excess of phosphates.

[Six months later Mr. Bryant told me that this patient's bones were still enlarging, and that there were evidences of enlargement of the skull.]

I have looked for records of cases similar to these in nearly every work that seemed likely to contain them, but

in vain. I have found only three cases, and the first two of these are doubtful.

Saucerotte¹ relates the case of a man who died at 40 and in whom all the bones, those of the head, face, orbits, ribs, vertebræ and limbs, had begun to enlarge about seven years before death. He increased in weight from 119 livres to 168, wholly from increase of bones; he had rheumatic pains; for a time sleepiness, oppression at the chest, and very small pulse; but these passed-by and he died with some acute illness. No examination was made.

Rullier² tells of a man, aged 78, who died in the Hôtel Dieu of empyema. He had previously been in good health, and nothing had indicated any derangement of cerebral function. The skull was very large, osteoporotic, and heavy, and, except the lower jaw, all the bones of the face were healthy. The ribs were thicker and larger than usual; the sternum narrow and very thick; the pelvic bones changed like those of the skull. The clavicles were thick, curved, and solid. The other bones were healthy.

Wraný³ has fully described the condition of the bones in a case of spongy hyperostosis of the skull, pelvis, and left femur, taken from a woman 50 years old, of whom, however, nothing is told but that she died of pyæmia, and that she had 'spongy hyperostosis of the skull with atrophy of the facial skeleton, spongy hyperostosis of the vertebral column, pelvis, and left femur, with elongation of the latter bone; kyphoscoliosis of the upper dorsal part of the spine; pelvic abscess; emphysema and œdema of both lungs, abscess of the left; marasmus.'

¹ *Mélanges de Chirurgie*, Paris, 1801.

² *Bulletin de l'École de Médecine de Paris*, t. ii. p. 94, 1812.

³ *Prager Vierteljahrschrift*, 1867, B. i. p. 79.

I cannot doubt that this disease was the same as I have here described, and the paper is valuable, both for the many signs indicated in it that the bones softened and yielded to pressure in the early part of the disease, and for the careful comparison of the distortion of the pelvis with the dissimilar distortions in rickets and mollities ossium. The spine was very curved; the chest small and too arched; the whole trunk very short.

From these cases which, though few, are well marked and in some chief points uniform, as well as from a recollection of two more of which I have no notes, I think we may believe that we have to do with a disease of bones of which the following are the most frequent characters:— It begins in middle age or later, is very slow in progress, may continue for many years without influence on the general health, and may give no other trouble than those which are due to the changes of shape, size, and direction of the diseased bones. Even when the skull is hugely thickened, and all its bones exceedingly altered in structure, the mind remains unaffected.

The disease affects most frequently the long bones of the lower extremities and the skull, and is usually symmetrical. The bones enlarge and soften, and those bearing weight yield and become unnaturally curved and mis-shapen. The spine, whether by yielding to the weight of the overgrown skull, or by change in its own structures, may sink and seem to shorten with greatly increased dorsal and lumbar curves; the pelvis may become wide; the necks of the femora may become nearly horizontal, but the limbs, however mis-shapen, remain strong and fit to support the trunk.

In its earlier periods, and sometimes through all its course, the disease is attended with pains in the affected

bones, pains widely various in severity and variously described as rheumatic, gouty, or neuralgic, not especially nocturnal or periodical. It is not attended with fever. No characteristic conditions of urine or fæces have been found in it. It is not associated with syphilis¹ or any other known constitutional disease, unless it be cancer.

In three out of the five well-marked cases that I have seen or read-of cancer appeared late in life; a remarkable proportion, possibly not more than might have occurred in accidental coincidences, yet suggesting careful inquiry.²

The bones examined after death show the consequences of an inflammation affecting, in the skull the whole thickness, in the long bones chiefly the compact structure, of their walls, and not only the walls of their shafts but, in a very characteristic manner, those of their articular surfaces.

The changes of structure produced in the earliest periods of the disease have not yet been observed, but it may certainly be believed that they are inflammatory, for the softening is associated with enlargement and with excessive production of imperfectly developed structures, and with increased blood-supply. Whether inflammation in any degree continues to the last, or whether, after many years of progress, any reparative changes ensue, after the manner of a so-called consecutive hardening, is uncertain.

The inflammatory nature of the disease is evident also

¹ There has not only been no history of syphilis in any of the cases, but no known syphilitic changes have been observed in any patient.

² See, also, Sandifort, quoted at p. 175; Museum of St. Bartholomew's, ser. i. 111 and 112, sections of a femur, large, curved, porous, with a tumour growing around its shaft; and 49, a hyperostotic skull from a man who died with cancerous disease of the eyeball, heart, and other organs; and Museum of Guy's Hospital, specimens of symmetrical osteoid cancer of the ilia, with cancer of the spine and cranium, associated with hypertrophy of the cranium. Dr. Goodhart was so good as to give me a report of this case.

in the changes of minute structure in the affected bones.¹ On these Mr. Butlin writes, 'With regard to the nature of the process by which these changes were accomplished, there are probably only three things which could produce so great an increase in the size of a bone, namely, new growth (tumour), hypertrophy, and chronic inflammation.

'The first of these may be at once set aside as out of the question.

'Nor is the second much more probable than the first, for the process is evidently no mere hypertrophy. The whole microscopical architecture of the bone has been altered; the structure appears to have been almost entirely removed and laid down afresh on a different plan and in a larger mould.

'Of the three causes chronic inflammation alone remains, and upon examination one or two facts will be found to bear strongly upon the theory of this being essentially an inflammatory disease. Not only the absorption of the old structure which has taken place, but also the manner of this absorption, point to its inflammatory nature. Traces of this are not, of course, always discernible, as the process is almost everywhere far advanced. But still careful observation not uncommonly discovers that the sides of the widened canals, instead of being smooth and even, are eaten out in a series of curves or concavities with the production of what are called Howship's lacunæ, so characteristic of inflammation. The tissue contained in the canals, too, almost precisely resembles the tissue found in the spaces of inflamed bones, only differing from it in being generally more fibrillar and less rich in cells, a fact easily to be accounted for by the very long duration of the disease and the general tendency towards organisation which was displayed throughout.

¹ And this is also the opinion of Wrang, *l. c.*

The apparent cessation of the process of absorption and the gradual process of repair may be regarded as still further leading towards the same conclusion.

‘Further than this the microscopical observations do not extend.’

The chemical analysis by Dr. Russell may be regarded as confirming this conclusion. It shows, at least, that there is no such change of composition in the bone as would be expected in any merely degenerative softening.

Holding, then, the disease to be an inflammation of bones, I would suggest that, for brief reference, and for the present, it may be called, after its most striking character, *Osteitis deformans*. A better name may be given when more is known of it.

It remains that I should point out the distinctions between this disease and the several forms of hyperostosis, osteoporosis, and other diseases among which it has been confused.¹

1. Among cases of hyperostosis are included those of simple overgrowth or hypertrophy of bones in adaptation to increase or change of office. The distinction of these from any form of disease is plain enough; they show a mere increase of natural structure.²

2. Scarcely different from these and as easily distinguished are the hyperostoses, best seen in the skull, in which the bones have more than normal thickness, hardness, and weight, and marks of greater vascularity, yet preserve a just relation of their several parts and a scarcely changed structure. They probably illustrate the effects of simple inflammation of bone recovered from.³

¹ Many of the statements here made are derived from the examinations of the collections of diseased bones in the College of Surgeons and St. Bartholomew's Hospital, which I made while writing the catalogues of their pathological museums.

² Mus. Coll. Surg., 379, 380, 2838, 2839, 2842, 2843, &c.

³ Mus. Coll. Surg., 2840, 2841.

3. A group of hyperostoses consists of those cases in which bones are enlarged in consequence of an increased supply of blood or lymph. Such a case is that recorded by Dr. Day¹ in which the bones of a boy's limb with obstructed lymphatics are much longer than those of the sound limb;² and such are all those in which bones near inflamed joints, or with partial necrosis, or in limbs long hyperæmic, from whatever cause, grow in length and circumference till they considerably surpass the bones of the healthy limb.³ These are easily distinguished. They have not signs of disease proper to themselves; they occur in the young alone; they may present a healthy texture, or one only slightly changed as by partaking of the adjacent inflammatory process; and with the exception of the tibia they do not become deformed. The tibia, when it lengthens more than the fibula, is almost compelled to curvature by the fixed unyielding attachment of its ends;⁴ and the curve is usually similar in shape and direction to the curve of the tibia in the osteitis deformans. But there is no other likeness between the two conditions.

4. A very large number of cases of hyperostosis are consequences of inflammations of bone; some of simple inflammation, others of scrofulous, syphilitic, or gouty inflammation. It is not necessary here to distinguish

¹ Transactions of the Clinical Society, vol. ii. p. 104, 1869.

² Broca, *Des Anévrysmes*, 8vo, p. 76, 1856, gives a case of femoral arterio-venous aneurism attended with considerable elongation of the limb.

³ I believe these were first described by Mr. Stanley, *On Diseases of Bones*, p. 20, *et seq.*, and myself, *Lectures on Surgical Pathology*, p. 64, ed. 3, and in the catalogues already referred to. Langenbeck has published a very interesting paper on them in the *Berliner Klin. Wochenschrift*, 1869, No. 26. Cases are also cited from Weinlechner, Schott, and Bergmann, in Virchow and Hirsch's *Jahresbericht für 1869*.

⁴ Such curved tibiæ are in the Museum of St. Bartholomew's, Nos. A. 3, A. 46.

these from each other,¹ but there are sufficient signs for the distinction of all from the osteitis deformans.

It is clear that the summary which I have given of the clinical characters of this osteitis would not tally with that of any case of simple osteitis, such as might ensue in a healthy person after injury, or in the neighbourhood of a sequestrum ; and the clinical difference is as complete between it and any case that could justly be regarded as strumous or syphilitic or gouty osteitis.

The anatomical differences are as well marked : chiefly in the facts that in these inflammations the bones do not become curved² (unless in the case of the tibia already explained) ; that they commonly display much more considerable external periosteal outgrowths or deposits, as if from a greater participation of the periosteum in the inflammatory process ; that the rarefied or, it may be, porous structure of the swollen shafts of bones usually shows appearances of separation and expansion of the component layers ; that the medullary canals are commonly invaded by the thickening walls, or are as much changed as the walls themselves ; that the whole length of a bone-shaft is very rarely affected ; and that the thin articular layers of bones are, I believe, never thickened as they are in the osteitis deformans.³

It may be added that it is very improbable that any form or degree of scrofula or syphilis or gout should exist in bones or any other textures for ten or more years without affecting other parts and without impairing the

¹ An attempt to do so is made in the pathological catalogue of the College of Surgeons.

² The absence of curving in bones around sequestra is remarkable, for they are long and often acutely inflamed, and those of the lower limbs are commonly used and bear weight.

³ Among the specimens in which these changes may be studied are, in the College Museum, Nos. 3085, 3089, 598, 3090, 3091 ; in the museum at St. Bartholomew's, A. 1, and ser. i. 56, 132, 138, 196-198.

general health. The retention of good general health during many years of localised disease is, indeed, one of the most striking characters of the osteitis deformans. The only parallel known to me is in the rheumatoid or chronic rheumatic arthritis, and the likeness between the two in this respect may suggest that they are nearly related; yet they are not found concurrent. In the case that I have related the amount of chronic rheumatic arthritis was trivial, and (which is more important) in all the records and specimens of the arthritis which I have seen I have not found an instance in which there were any of the morbid changes characteristic of the osteitis.¹

5. There are, I think, only two other diseases, namely, rachitis and osteomalacia, from which it can be necessary to discriminate the osteitis deformans, and the differences between them are very wide. They have scarcely a feature in common except that in all of them the bones bearing weight become curved or mis-shapen, and the spine is usually deformed, and the skull may become very thick and porous. But in rachitis the bones are too short, not too long; too small, not too large; and their curvatures are quite unlike those of the osteitis. And in the osteomalacia the walls of the bones become exceedingly thin, wasting with an acute atrophy; and when they yield it is not with regular curving but with angular bending or breaking. By these and many other differences, as well clinical as anatomical, the diagnosis of the osteitis from rachitis and osteomalacia is sufficiently clear. With rachitis it may be judged to have no affinity whatever; with osteomalacia only so much as may exist between a chronic inflammation and an acute atrophy of any part. Yet by one character which all these three diseases have

¹ There is not even any mention of them in Mr. R. Adams's elaborate *Treatise on Rheumatic Gout*, 1873, 8vo and folio.

or may have in common, namely, the osteoporosis of the skull, they are constantly confounded in museums, if not in practice, with each other, and with diseases different from them all.

The study of the osteitis deformans led me to learn what I could of the various recorded descriptions of large, thick, and porous skulls often found in museums. Nearly every large museum contains one or more specimens of such skulls whole or in fragments. They are all big, thick, porous, or spongy, with obliterated sutures and wide apertures and grooves for blood-vessels. Very few of these specimens have any life-histories; they are all, in many respects, alike and usually are all named alike. Many of them it may be impossible to name or classify without much better knowledge of them than may now be had, but I believe that among them are the results of several different diseases; and it may save some trouble to future students if I refer to some of the specimens and records which have led me to this belief.

1. Some are examples of the osteitis deformans which I have described.¹

2. Some are derived from cases of osteomalacia. Mr. Durham² has written on these, and Mr. Solly's³ well-known paper gives a good instance of them. In general, I think that these may be distinguished, at least in the recent state, by their softness and lightness; the

¹ To those already referred to these, I think, may be added: Sandifort, 'Museum Anat. Acad.'; Lugd.-Bat., fol., 1835, vol. i. p. 142, vol. ii. tab. xiii., Skull of a man 43 years old, with a 'fungus' over the left orbit (? a cancerous growth). Other similar skulls are here referred to. Similar specimens are, probably, Nos. 2840 and 2858A in the College Museum; and, more uncertainly, 2841 and 2858, which, perhaps, belong rather to the fifth group.

² *Guy's Hospital Reports*, ser. iii. vol. x. 1864.

³ *Med. Chir. Trans.*, vol. xxvii. p. 435; *Mus. Coll. Surg.*, 395.

abundance of soft medulla contained in them, and the comparative brittleness of the bones when dry.

3. Some are from rachitis; they are, unless after recovery and repair, very light, almost friable, and on their surface not porous, but like fine cloth or felt.¹ Like these are the skulls of some lions and monkeys which have died young, in confinement, of what is considered rickets. A collection of these skulls and other similarly diseased bones in the College Museum² deserves careful study, especially because of their likeness to the cases included in the next group.³

4. These are the results of a disease of early life, sometimes even of childhood, in which all the bones of the face as well as those of the cranium are affected, and, it is said, the bones of the limbs. All the affected bones, facial as well as cranial (and herein is a clear ground of diagnosis), become hugely thickened, porous, or reticulate. The whole skull is very large, clumsy, and featureless. Commonly the cranial cavity is diminished. The orbital and nasal cavities are contracted, the antra are often filled, by the ingrowth of their several walls; the apertures for nerves are narrowed or obliterated.⁴

Of these cases, which are among those named by Virchow⁵ *Leontiasis ossea*, the best are related by Ilg⁶

¹ See Mus. Coll. Surg., 390-394, 2844, and 2857. I believe that Huschke, *Ueber Craniosclerosis*, 1858, quoted by Virchow, contains facts on the rachitic osteoporoses, but I have not been able to refer to it.

² Nos. 386-388, 2854-2856, 2855A, &c.

³ Although bones such as these are not described by Paul Gervais, yet his paper quoted below should be studied on all that relates to hyperostosis in animals.

⁴ Among the casts in the museum at St. Bartholomew's, No. 10 is that of a skull affected with this disease, and in ser. i. 36, are fragments of a bone, which, I think, may be referred to it.

⁵ *Die krankhaften Geschwülste*, B. 11, 1864-5. I need not say that this contains a very complete account of all forms of overgrowth of bone.

⁶ *Einige Anatomische Beobachtungen*, 4to, Prag, 1821.

and Jadelot.¹ Their descriptions are very scanty, yet they give sufficient facts to distinguish their disease by their account of the cerebral symptoms associated with it. In Ilg's case, for example, the patient who died at 27, after seventeen years' disease, had amaurosis, epilepsy, severe general headache, delirium, convulsive attacks, and at last total deafness, witlessness, difficulty of swallowing, and loss of smell.

5. Some cases, perhaps not different from these, though they have occurred in later life, are those by Schützenberger,² Otto,³ and Wraný.⁴

6. And, lastly, there are cases not so much of thickening of the cranial and facial bones as of enormous bossed and nodular hard bony outgrowths overspreading them or projecting from them. The leading case among these is that published in the Transactions of the Pathological Society by Dr. Murchison,⁵ with a report on the specimens by Mr. De Morgan and Mr. Hulke.⁶ The

¹ Quoted by Ilg from Meckel. The best of many accounts of this specimen is given by Paul Gervais, 'De l'hyperostose chez l'homme et chez les animaux,' in the *Journal de Zoologie*, t. iv. 1875. He has carefully re-examined the skull and face and described them.

² *Gazette Médicale de Strasbourg*, and in Canstatt's *Jahresbericht für 1856*, B. iii. 34, with references to cases by Breschet and Nélaton.

³ Otto, *Neue seltene Beobachtungen*, 4to, 1824, p. 2. Both head and face are affected; the bones are described as, after softening, very hard, dense, and almost ivory-like. Six hyperostotic skulls are mentioned in his *Neues Verzeichniss der Anat. Sammlung zu Breslau*, 1841.

⁴ Wraný, 'Hyperostosis maxillarum,' in *Prager Vierteljahrschrift*, 1867, B. 1, similar affections of the facial and cranial bones, with cerebral symptoms. Doubtful cases by Ribelt are quoted by Ilg, *l. c.*; Malpighi, *Opera Posthuma*, 4to, Amstel., 1700, p. 68; Kilian, *Anat. Unters. über den neunten Hirnnervenpaar*, Pesth, 4to, 1822, p. 133; Quekett, reported by Hewett, *Medical Times and Gazette*, Sept. 8, 1855, p. 229.

⁵ Vol. xvii. 1866, p. 243.

⁶ Similar cases are illustrated by Forcade, quoted in Virchow's *Die krankhaften Geschwülste*, B. 2, p. 22; Weber, from a specimen in the Dupuytren Museum, in v. Pitha and Billroth's *Handbuch*, B. 3, Abth. 1, Lief. ii. p. 257; Howship, *Practical Observations in Surgery and Morbid Anatomy*, 1816, p. 26; Adams in *Trans. of the Pathological Society*, Vol.

disease in which the facial more than the cranial bones are affected is clearly distinct from any of the foregoing, or if it be in any way connected with them, especially with those of the fifth group, may be regarded as transitional from them to the exostoses, especially the massive tuberos and bossed ivory exostoses, which grow on or among the bones of the face and skull. The same approach to the character of hard exostoses is shown in the disease of the fibula in Dr. Murchison's case, a section of which, from the museum of the Middlesex Hospital, is now before the Society.

xxii. p. 204, 1871; Lysthay, in Canstatt's *Jahresbericht für* 1858; Mus. Coll. Surg. Eng., 3093. Virchow has a full account of nearly all these cases, and of the analogies of the disease with elephantiasis of soft parts.

X.

HUNTERIAN ORATION, FEBRUARY 13, 1877.

MAY it please your Royal Highness : Mr. President, my Lords, and gentlemen,—I do not doubt that I may offer to your Royal Highness the thanks of the whole College of Surgeons for your presence here to-day. By thus honouring the memory of John Hunter, you make us more than ever proud of being the guardians of his great museum and his reputation ; you make us more than ever anxious for the promotion of that true scientific surgery of which we reverence him as the founder. Moreover, we shall venture to regard your Royal Highness's presence as a sign of your approval of the efforts of the College for the public welfare, and of your desire to encourage the sciences on the cultivation of which its reputation and utility depend. For these, and many more unspoken reasons, the College, fully represented here to-day, offers to your Royal Highness its grateful and respectful thanks.

When time and the favour of my colleagues in the Council brought to me the occasion of delivering the Hunterian oration, I thought it right to study afresh the character of John Hunter. And now I beg your leave to tell some of the facts and thoughts to which, in my study, I have been led—chiefly to tell, if I can, what were the motives of John Hunter in his scientific life ;

what were the chief characters and what the method of his work ; to tell, also, some of his achievements, and of the lessons that may be read in the story of his life.

I hope that I may thus fulfil, however imperfectly, the design of the founders of the oration, by promoting the honour of John Hunter and, perhaps, even the progress of surgery, by showing, in his illustrious example, the good influence of the scientific mind.

The motive which first urged John Hunter towards the pursuit of science seems to have been only the necessity of earning his livelihood. For we find him, at first, as the youngest child of a Scotch laird, idle and negligent of education. In the first twenty years of his life he appears to have had no inclination to science or to the arts that minister to it, or, indeed, to any kind of intellectual pursuit. We find no tales of early enterprise, no childish love of nature, no sign of future mental power. When he was seventeen, he tried to help a brother-in-law, who was a bankrupt cabinet-maker in Glasgow ; and, probably, if he had succeeded, cabinet-making might have been the business of his life. Happily, he failed ; his brother-in-law was past helping. Then, after two years more of idleness, what was to be done ?

His brother William Hunter, ten years older than himself, was prosperous in London, and was becoming distinguished as a teacher of anatomy and surgery ; so he offered to assist him, and if this should fail he would go into the army. Thus, in mere idleness or necessity, with no other reason than that there seemed nothing else to be done, John Hunter drifted into the opportunity of scientific study—drifted into the career in which he was to become great among the greatest men of science, and among all surgeons of all times the most renowned.

It seems strange that a mind so remarkable as John Hunter's, so robust and self-willed as it proved, should not have shown or felt its power till, as if by chance, it was brought to scientific work. He had not lived in darkness or among dull people : his father was a shrewd and sensible man ; his mother well educated ; his two brothers were persons of remarkable mental power. With these, his mind had had opportunities of exercise and culture ; but he had neglected them as to him useless. He had lived among the same wonders of the organic world, the same truths and utilities in nature as moved him, in his later years, to restless study ; yet he seems to have given no heed to them. No desire of knowledge was stirred in him till he was under the influence of scientific minds.

It may be that his mind only now began to attain the maturity requisite for a desire of scientific knowledge. But I think it was rather that now, for the first time, he found, in the company of his brother, the subjects and the method of work for which alone his mind was naturally fitted.

In 1748, when John Hunter came to London, there was great intellectual activity in all the medical sciences, and William Hunter was in the midst of it. He was an intimate associate with some of the best minds of the day ; he was the best lecturer and best anatomical teacher of that time ; fluent, well-read, devoted to science and art, an earnest observer, an enthusiastic collector, willing to spend all that he could earn on his museum and his means of teaching. William Hunter, indeed, may be counted as the first great teacher of anatomy in England, the founder of the first great school, and among the biologists of his time and country second to none but his brother.

To be taken from idleness on a Scotch farm to an activity of life, such as John Hunter found in his brother's school, was like being born into a new world ; and this was the very world, if not the only world, in which the best part of his mind could live and grow. He had a natural fitness for the study of living things ; for other things he seems to have had no more desire or capacity of knowledge than common men have. The germ of this fitness was, we may believe, born in him, as in both his brothers ; and when, at length, it found appropriate conditions of its life, it developed and grew into true grandeur. But this natural fitness was wholly intellectual ; there was, at first, no love or desire in it ; and so his mind had no motive power till it was set to its right work, and in right working found happiness.

For the happiness of intellect is in its work ; that of the highest intellect in vigorous self-guided work. The highest intellect finds that happiness, the desire of which is its energising motive, not in the mere reception, nor wholly in the possession of knowledge, but in the process of acquiring it and of using it in thoughtful exercise.

Moreover, to some intellects, and among them many of those by which the greatest results in science have been attained, there is but one kind of knowledge which satisfies either in the getting or in the having. To John Hunter there seems to have been no great intellectual happiness except in the study of living nature ; and to this he was now first brought. Hence onward there was no lack of motives. The mind that had been idle, heedless and aimless had come to its right field of action ; every form of intellectual exercise and pleasure was offered to it, and it grew to capacity for all. Gradually the desire of knowledge, both for its own sake and for the

happiness of gaining it and using it, became like an insatiable passion, a motive to incessant work.

Now, I believe that in Hunter may be studied an example of the influence of the simplest and most natural motive of the scientific life; namely, the intense desire for the happiness of knowledge and of intellectual exercise in watching and working for the truth. It is not mere curiosity. Curiosity is, indeed, a necessary part of scientific desire; but it is not the common curiosity which can be satisfied by hearsay; which seeks only new things, careless whether they be true if only they be wonderful or personal, or whether, if true, they can be wrought into real or useful knowledge. Hunter is a type of the true men of science, in that he was always impelled by desire to attain knowledge by intellectual self-exertion. And, like an athlete, restless unless in the exercise of his strength, so he could not rest; he could not but search, and watch, and question Nature; he must compel her to answer, and then compare and interpret her answers and penetrate to their inmost meanings. And he could set no limit to his search. Within the range of the great world of life he must needs seek, by every method of enquiry, every kind and degree of knowledge.

With this passion for knowledge of biology another concurred. Hunter had a passion for collecting. It may have come through imitation of his brother; it may have been a mere yielding to the fashion of the time, as dominant then as it is now; but I believe it was natural—an instinctive love of gathering and keeping; and it was vehement in him, and worked together with his desire of knowledge, each animating and provoking the other.

It cannot be maintained that Hunter's love of collecting was only consequent on his desire of knowledge. Science determined its first direction; and the great

desire of his life was to have a grand museum, with ample and costly illustrations in catalogues and drawings. He would have collected, if he could, everything that could show to himself and others all the great facts in biology that he could find. But even this could not satisfy his love of collecting; for, besides his museum, he collected a crowd of things that must have been useless even to himself, and must have helped to make him poor; pictures of much cost, engravings, armour, works of art in ivory, bronze, marble, stuffed birds, and implements of savage warfare. With all these his house in Earl's Court must have looked like a curiosity-shop. But if this were a fault it may easily be pardoned; no earnest collector ever binds himself within the limits of science, utility, or prudence. And if one would justify an extravagant love of collecting, silly as it often is, the example of Hunter might be urged. It led him to constantly larger ranges of enquiry; it was among the motives of his widest and deepest studies, and it incited the industry and skill with which he gathered the great stores of facts which are treasured in this College. A century of study has not exhausted all the truths that are contained in his collections; and gathered round them, as if by the attraction of a central force, is the museum of the College, now more than twice as large as the Hunterian, and forming with it the very thing that Hunter longed for—the best anatomical museum in the world; the most perfect in order and condition; the largest treasury of visible biological facts.

One more motive of Hunter's scientific life must be told. He was a master in all the arts of anatomy; very skilful in dissecting, injecting, and all known methods of displaying specimens. I suspect that his first success in life was in his first dissection; and it is said that he

was so fond of his art that it was among the motives which led him beyond the study of human anatomy into that of comparative anatomy, which, till his time, had hardly been studied in this country.¹

These, I believe, were the special motives of Hunter's scientific life; and they deserve study, for his life was given to science as entirely and as purely as that of any man.

Doubtless we may discern in him how, sometimes, other motives added to these their various force. But they were casual and wholly subordinate. They were far outweighed by the always growing power of those motives of his life which I have described and, chiefly, by the desire of happiness in intellectual exercise. This desire increased with indulgence, with contest against difficulties, with the view of constantly new objects, and with the encouragement of success. He filled himself with knowledge, and, through knowledge, became an ardent lover of nature. I say, through knowledge: for Nature, in her manifold perfections, inspires many kinds of love; and Hunter's was almost wholly intellectual. He had none of the love that moves the poet, the idealist, or the theologian: for, in truth, neither poetry, nor idealism, nor theology ever coloured the simplicity of his scientific mind. He had the social love of nature; he was a warm-hearted man, loved dogs and horses, and sometimes writes of the living things about him as if they were companions. But his chief love was for the charms of truth that lie hidden beneath the appearances, the veils, of nature; and his love was enhanced when search revealed the utility of all he saw—the perfection of the adjustment of everything to its use; the evidence of purpose fulfilled in every change; the

¹ Norris, *Hunterian Oration*, 1817, p. 49, and *Ottley's Life of Hunter*, p. 7.

evidence of grandeur in a world of infinitely various forms held steadfast by few laws.

In all these he found delight and motive for fresh study; and I cannot doubt that he attained that highest achievement and satisfaction of the intellect when it can rest in a loving contemplation of the truth; loving it not only because it is right, but because it is beautiful. I cannot doubt that in the contemplation of the order and mutual fitness in a great field of scientific truth there may be, to some high intellects, a source of pure delight such as are the sensuous beauties of nature to the cultivated artist-mind, or virtue to the enlightened conscience. I believe that in contemplation such as this Hunter enjoyed a calm, pure happiness. So Reynolds, his friend, seems to tell of him. In that masterpiece of portraiture, which teaches like a chapter of biography, Hunter is not shown as the busy anatomist or experimenter pursuing objective facts; the chief records of his work are in the background; he is at rest and looking out, but as one who is looking far beyond and away from things visible into a world of truth and law which can be only intellectually discerned. The clear vision of that world was his reward. It may be the reward of all who will live the scientific life with the same devotion and simplicity.

Let me speak now of the chief characters and the method of Hunter's work, and thus try to indicate the character of his mind.

That which first and always strikes one is the vast quantity of work he did. It is told of him by one pupil that 'he rose regularly at the dawn of day, and never ceased from his labours till the night was far advanced'; by others, that he allowed himself only five hours for

sleep; by another, that when he gave him a letter of introduction he was asked to call at five the next morning, and found him already at work in his museum.

Such as these were Hunter's habits during at least the last thirty years of his life; and it was not in busy idleness that he spent this time. Counted in mere quantity, very few have left so large results of scientific labour as he did. Besides the four published volumes of his works, he left a vast number of manuscripts written or dictated by himself. He dissected more than 500 species of animals, and of some of these many different examples. Of these dissections he left descriptions of more than 300; and besides these were all his studies of human anatomy, and the dissections of hundreds, or even of thousands, of diseased structures. His museum contained nearly 14,000 specimens, and all these he had either prepared or at least personally and closely studied. Nearly the whole of this work was done in thirty years: during all that time he was active in the practice of surgery; and as he grew older he only worked the more. His latest letters are more than ever full and urgent to his friends to send him everything from which he could gather knowledge—not merely things rare or wonderful, but whatever could be studied, whatever could yield facts for clearing or enlarging his view of life.

Even his amusements, as he calls them, were what idle men would call hard work. 'I amuse myself,' he says, 'with bees'; and the results are told in essays which one of the best recent writers calls, 'almost faultless.'¹ They are full of minute observations and of careful and ingenious experiment and thought; they show that habit of his mind in which it always watched small

¹ E. L. Ormerod, *Natural History of Wasps*, p. 7.

things, as if they might indicate great laws; they might alone have gained for him a good scientific reputation.

Or, in evidence of the quantity of work that he would devote to one investigation, hear what he says of his observations on the development of the young bird within the egg. For the purpose of observing it better than he could, though he made many trials, in the eggs of chickens, he says: 'I kept a flock of geese for more than fifteen years, and by depriving them of their first brood in my investigations, they commonly bred again the same season.' And: 'As hours make a difference in the first days, it becomes necessary to examine in the night as well as in the day, by which reason the latter brood in the summer is best adapted, having then short nights.'¹

Surely one might suppose that this was the great work of his life; this hourly occupation by day and night for parts of fifteen years. Yet it seems to have been rather a casual by-the-way pursuit. He became, indeed, so charmed with the study of the young birds in eggs that he says: 'It would almost appear that this mode of propagation was intended for investigation'; and yet, though he attained knowledge far in advance of all before or with him, he did not publish it, and the extent of his researches was not nearly known till long after his death.

The range of Hunter's work matched with its quantity and the time devoted to it. Never before or since—I think I am safe in saying this—never before or since has any man been at one time a thorough student and investigator in so wide a field of science. He was an enthusiastic naturalist; as a comparative anatomist and physiologist he was quite unrivalled; among the pathologists of his time he was by far the first; among the few geologists and students of vegetable physiology he was

¹ *Essays and Observations*, vol. i. p. 206.

one, if not chief ; and he was a great practical surgeon, surgeon to a large hospital, and holding for some years the largest practice in this town. In all these subjects at one time, no one but Hunter has ever been eminent and active.

And it is not only in the range but in the depth and thoroughness of his scientific work that he is distinguished. It is not possible now to tell, by any examples, the thoroughness of his scientific work. Let me say only, that in the whole range of subjects which I just now indicated there was not one which he did not study as completely as was possible ; not one in which he did not enlarge the area of enquiry far beyond that covered by those before him. In every department of the sciences of life he made investigations wholly original ; he observed and recorded facts past counting ; he discerned in his facts large general laws.

These notes concerning Hunter's work tell the chief characters of his mind ; massiveness and grandeur of design appear in all he did ; and in perfect harmony with these was the simplicity of his usual method of work. It was, mainly, the orderly accumulation of facts of every kind from every source, and the building of them up in the plainest inductions. If he had been an architect, he would have built huge pyramids, and every stone would have borne its own inscription. He knew nothing of logic or of the science of thought ; he used his natural mental powers, as with a natural instinct—used them with all his might, but without art or consciousness of method. I know no instance more signal than was in him of the living force there is in facts when they are stored in a thoughtful mind.

But Hunter was not only a great observer ; he was a very accurate one. Among the masses of facts recorded

by him, it would be hard to find any that are erroneously observed or stated : when there are errors in his works they are errors of reasoning, not of observation.

And I note it as an exemplary instance of his accuracy, that when he tells his general inferences from facts, he habitually uses words implying that he regarded them as only probable. A fact he tells without conditions ; when he generalises, it is commonly with such words as 'I conceive,' 'I suspect,' 'I am disposed to believe,' or the like.

I think, too, that no instance can be found in which he tried to add to the strength of evidence by strong personal assertion ; as if his opinion were to be taken for weight in an estimate of probability. Nay, there are very rarely any expressions implying strong conviction on any large question in biology. No one, I think, knew better than Hunter that, in science, strong convictions are not usually the signs of knowledge. He seems to have always felt that, in the consideration of general principles, he had only reached near to the greatest probability attainable at the time ; that another year or more of investigation would bring him nearer the truth, and that which now seemed right would be surpassed or set aside. He used even thus to tell the pupils at his lectures : ' Don't take notes of this ; I dare say I shall change it all next year.'¹

It is another sign of this wise caution that he always hesitated to publish his knowledge. He worked for eighteen years before he published anything in his own name. He was forty-three when he published his first book, that on the Teeth ; he began to collect the materials of his great work on the Blood and Inflammation while he was a student ; some of the experiments recorded in it were made while he was house-surgeon at

¹ Similarly in *Ottley's Life*, p. 49.

St. George's; he worked at it for forty years, and began to print it only just before he died.

And he was as patient as he was cautious. Abernethy, who knew him well, says : ' It is scarcely credible with what patience Mr. Hunter examined the lower kinds of animals ' ; and he quotes Mr. Clift as saying, ' that he would stand for hours, motionless as a statue, except that, with a pair of forceps in either hand, he was picking asunder the connecting fibres of some structure ' he was studying. A very striking picture : for this was in the last year of Hunter's life ; he was growing old ; he had lately been very ill, and he knew that he was in instant peril of that sudden death in which, at last, he fell ; he was poor, for all that he could earn, and more, he needed for his collection ; and he was overworked in practice and in the duties of Surgeon-General to the Army. Yet ' he would stand for hours, motionless as a statue ' ; patient and watchful as a prophet, as if he were sure that the truth would come, whether in the gradual unveiling of new forms, or in the clearing of some mental cloud, or as in a sudden flash, with which, as in an inspiration, the intellectual darkness becomes light.

In all these indications of Hunter's character we may observe, together with grandeur in design, and a power and strong will well proportioned to his design, an unusual prudence in all the work of observation ; and yet he was very fond of scientific enterprise and speculation. The qualities may seem incongruous ; but they are associated in the most attractive minds, and may be traced in Darwin and others of the best of our own time.

Enterprise was shown in his devotion to experiment. If there were one class of facts which he loved better than all besides, it was of those which he could thus obtain. He seems to have had a very keen enjoyment of

that mental state in which is the very spirit of enterprise ; the state in which the mind waits, watching for the solution of a problem which itself has made, standing as it were in the presence of the about-to-be-known.

And, as he was always thinking-out beyond the facts which he could collect in the normal course of nature, always projecting his mind beyond his knowledge, so he made every question that he could the subject of experiment. He used to say to those who seemed content with thinking about what might be known : ' Don't think : try ; be patient ; be accurate.' ¹

But, where observation and experiment could not reach, few were bolder in thinking than Hunter was. His long practice in experiments justified him in this, by educating him for more distant mental enterprise ; for a well-devised experiment, such as many of his were, deserves the name of project, in that the mind, throwing itself forward in advance of present knowledge, believes that the truth must be in one of two or a few more probabilities, and then devises means for ascertaining where it is ; and a mind which has been strengthened and long trained in this kind of exercise may sometimes safely project itself much further, and, going beyond the range of experiment, may discern a general truth very far in advance of ascertained facts, or even from a stand-point of partial error. Hunter could sometimes, with very striking power and precision, thus think the truth.

Thus, for one example, he thought the truth that the blood is alive ; not in any supernatural or transcendent sense, as some before him thought, but in the same sense as are all other parts of the same living body. In this sense his discernment of the life of the blood was a real discovery ; not a guess, or, in the worser sense, a

¹ Baron's *Life of Jenner*, vol. i. p. 124.

mere scientific imagination ; for he saw fully the bearings of the doctrine, and it guided him to some of the first steps in his true pathology of the blood.

The truth is now proved beyond all doubt ; but if we look to the facts on which Hunter first founded his decision, they seem insufficient ; and we have to assign the discovery chiefly to the power of his strong, far-seeing mind looking out beyond all evidence.

Similarly, in his essay on the development of the chick, he indicates, though from what may be thought too few facts, that great and marvellous law in development, that every higher animal, in its progress from the embryo to the complete form, passes through a series of changes, in each of which it resembles the complete form of some order lower than itself.¹ And this discovery, like the last, was not a lucky guess, just made and then left. He saw nearly all its importance. It became a fruitful doctrine, guiding or confirming him in the assurance which determined a large portion of his studies, that, in the whole range of physiology, every lower form of life should be studied for the simpler and the elemental instances of the functions performed in the higher.

And I find at least one instance of his projecting his mind far into the doctrine of evolution, though this time fruitlessly. In writing on hermaphroditism, which he studied very carefully, he says, in a foot-note : ‘Query : Is there ever, in the genera of animals that are natural hermaphrodites, a separation of the two parts forming distinct sexes ? If there is, it may account for the distinction of sexes ever having happened.’²

It was natural that one who could thus discern great truths far off should strive for a decision on that question

¹ *Physiological Catalogue* by Owen, vol. i. p. ii.

² *Works*, vol. iv. p. 36. See also *Essays and Observations*, vol. i. p. 249.

in biology which, from the earliest days of scientific thought, has never ceased to be discussed—the question, What is Life? Hunter strove for an answer to it, and his opinion greatly affected his pursuits, and, much more, for a time, his influence and his reputation in medical science. He spoke of life as a ‘vital principle,’ a something separate from organisation; and, although he spoke also of a *materia vitæ diffusa*, and of a *materia vitæ coacervata* in the brain, yet I cannot doubt that he meant something that was not material or a mere property of matter. I believe that he intended by his ‘vital principle’ that which Joseph Henry Green, the most philosophic and eloquent of his interpreters, held—‘a power anterior in the order of thought to the organisation which it animates, sustains, and repairs; a power originative and constructive.’¹

But Hunter could not clearly express this and, I believe, he could not clearly think it. For it cannot be denied that on this and some allied subjects he wrote very obscurely—so obscurely and so variously, that when we have granted all we can for the common difficulty of finding words for profound thoughts, and all we justly may for our own defects of apprehension, we cannot but believe that his mind was not clear upon them. And this, I believe, we may refer to one of the few intellectual defects that can be traced in him—namely, the great inequality of his powers of language and of thought.

In every mind thoughts and words are so interwoven that each shares always the qualities of the other. Thoughts and words are like mutual reflectors: if either of them distorts an object placed between them, the other cannot but receive the distorted image and reflect it. Or each is, alternately, master and servant. Now thought

¹ *Hunterian Oration*, 1848.

employs words for its expression, and then these same words take part in directing the next thought. If either be defective or erroneous the other suffers.

Hunter was a great master of facts, and in plain and customary English he could with great power collect, compare, arrange, and construct whatever could be made from them ; but he was not a master of words. His large, strong mind does not in anything show that subtlety which, whether in thinking or in writing, can accurately employ many words of scarcely different meanings—a quality which is very necessary for the consideration of abstract ideas, and in which a defect is a hindrance, not only to the expression of thoughts, but to the process of thinking.

Hunter's defect in this respect may have been due, in part, to his neglect of early education ; but chiefly, I think, it was natural. In many other things he corrected all the faults that could be referred to neglected education ; in language, whether in speaking or writing, he was, to the last, deficient ; and his thinking power, strong as it may have been by nature, was hindered and baffled by its weak associate.

Nevertheless, however incomplete his thoughts on the vital principle may have been, he worked well with it as an hypothesis. With its help he threw off the fetters of the erroneous chemical and physical doctrines which, just before his time, were prevalent in physiology. It led him to larger and clearer views of the work to which he gave himself ; it was as a single band holding together all the objects of his study. Moreover, some of his pupils made the doctrine of a vital principle the chief ground of his reputation ; and though it was neither a new doctrine nor essential to the weightiest part of his teaching, yet it became a chief dogma of his school, and the discussion

of it served usefully, in a time of need, to keep alive his reputation and the praise of his great example.

But what seems to me, at this point, most instructive in respect of the character of Hunter is the just estimate which he made of the relative values of hypotheses and facts. His hypotheses, whether on the vital principle or on any other subject, never diminished his zeal for facts. He stands well that severe test of the strength of a scientific mind—the test of resistance against subjection to its own hypotheses. The feeblers men worship the works of their own minds; they fall down before their own idols made with words; they feel more sure of what they call their principles than of plain facts. It was not so with Hunter. He may have admired the hypothesis of a vital principle—and he used it wisely—but he much more admired the observation and right use of facts. He collected them as with an avarice; he kept them pure, in memory and manuscript; compared, arranged, and read in them, as he could, the laws of life. Herein was the principal, the best, the most abiding of his works; hence was his great influence in science.

But in Hunter's mind, careful in observing, bold in speculating, we have an epitome of the natural course and temper of biology. It is eminently a science of observation; and yet none who love to think can study the phenomena of life without asking themselves, What is life? and, still beyond this, Whence is life derived? 'An imperious instinct commands us to look beyond or beneath the phenomena'; we cannot believe that it is impossible for us to reach far beyond the evidence of sense; and even when beyond the phenomena there are discerned, as we believe, forces measurable and correlated, still, we cannot rest here; for the knowledge of the manner in which forces act tells nothing of their origin;

and this, especially in respect of life and mind, is what we earnestly desire to know. Whence comes the force of life? Is it 'a power anterior to the organisation'? Is it 'a power originative and constructive'?

I do not doubt that in a doctrine of vital force as correlated with physical forces we are nearer to the truth than in the doctrine of the Hunterian School, that life is wholly unlike and alien from other means and methods of activity. But the holding of the correlation and mutual conversion of the physical and vital forces does not determine the precedence of either the one or the other. If they are mutually convertible, either may have preceded; a vital force may have preceded the physical, though life was manifested very late upon this planet in any of the phenomena in which we now can study it.

And even if we were to hold the conversion of physical or vital force into consciousness and will—though from this what I believe to be my consciousness and my will are utterly repugnant—yet this would not prove the precedence of the physical force. The opposite conversion can be as well, or as ill, traced. Therefore, mental force, self-consciousness, and will, may have preceded other forms of force; mind may have existed before any of the properties of matter; and thus, even in the view of science, the first essence may have been a Being willing and knowing, and this Being may have been the prime source of all the forces whose operation we now trace.

There is nothing, I believe, in science to disprove such a belief as this; but I doubt whether it is within the power of science alone to determine certainly an order of precedence among forces. I cannot conceive of anything beyond or before a natural force except a supernatural will; and it is a fact to be weighed, that a belief such as this is held by untutored minds, as if it were instinctive

knowledge. Man seems naturally prone to think that above all that appears in the world there must be a Mind or minds, in the image of which his own is made, and with which he is in some kind of personal relation. But the proof of these things is not yet reached by science; and till it reaches proof, science cannot rest, must not rest. But the firm and life-guiding belief that a supernatural Will and Knowledge was, and is, and will be, and the lessons of our personal relation to It, may rest secure on the whole and manifold evidences of the Christian faith. They sometimes are in conflict with what we hold to be true in science. Then let us wait, and strive that where there cannot be concord, there may, at least, be truce. Time or, if not time, eternity will show that science and Christian theology are two sides of truth, and that both sides are as yet only known in part.

But I leave this, which may seem a too far digression, that I may revert to that one part of Hunter's mind which remained, with little change, from his days of idleness in the Scotch farm: I mean his unconsciousness of his mental power.

He could be provoked into saying that he knew himself to be better than some of those who spoke ill of him, but he declared that he felt 'a mere pigmy' when he looked at the work to be done. Even the sensitive vexation with which he sometimes spoke of rivals in surgery is enough to prove that he was unconscious of having done work great enough and good enough to win the highest fame. He appears as one desiring renown, but doubtful whether he had gained or deserved to gain it. As he bears other tests of mental greatness, so he bears this—the test of self-unconsciousness. And it was happy for science that it should be so. For if Hunter had thought of himself as we think of him, wherein we must hold that he would

only have thought justly, he could scarcely have failed to become self-enamoured, and he would have lost time in trying to select work adequate to the grandeur of his mind, and to set his mind in attitudes attracting a just homage. And, according to his own judgment, he would have failed ; for, as he wrote, with even more than usual disparity of wisdom and of words : ‘ There never was a man that wanted to be a great man ever was a great man.’

And now, what did he achieve ? What influence did he exercise on the progress of knowledge ?

We have seen that his work was various : so were its consequences ; and in these we may discern, I think, a striking illustration of the well-known rule, that, before great truths can be taught, the minds of a people or of a society must have attained a certain capacity and desire to learn, and that the capacity and desire must be, not general, but with special fitness in the subject of the new teaching.

Hunter studied living things as well in disease as in health. In both states he saw the same power and the same general design, though in diseases often overborne ; and, in his view, the differences between disease and health were conditional, not essential. They were to be studied as closely related parts of one science of life. But he had too much common sense not to see the broad practical differences between health and disease. He taught them separately, wrote of them separately, and in the study of his influence and reputation we must make a similar separation.

Hunter’s greatest work was in physiology. Using that word in its fullest meaning, as the whole science of the normal life of all things that live or have lived, he grasped this science with the widest mental grasp. In

his study of comparative anatomy and physiology, which he may justly be said to have introduced into this country, he saw and showed the way to the whole study of life; and this he did as of his own force. He neither followed others, nor merely drew a plan on which other men might work; but with his own mind he planned and with his own hand he wrought, a larger and a truer work in the whole science of living things than any man before him. And in this work no man succeeded him.

His lesser work, great as it was—greater, I think, than that of any man before or since—was in pathology; yet of this lesser work the direct influence was far greater than that of his greatest achievements in physiology.

For from among his pupils there went out all the great English surgeons of the time next after him. Abernethy, Astley Cooper, Cline, Home, Lynn, Blizard, these all learned of him, and all were chief teachers of surgery in chief schools of London: they boasted of being his pupils, they taught in his method, and it became a tradition in their schools. Hunter was thus, in the fullest sense, the founder of a school of surgery. But among all his pupils there was not one who gave himself to physiology, not one who studied it as he did, not one who continued the great works he left unfinished.

And although Hunter was an active and influential fellow of the Royal Society, a friend and companion of the leaders in science, the founder and patron of a scientific society,¹ in high repute as the chief anatomist and naturalist of his time, yet in his great work of comparative anatomy and physiology not one of the younger men of science imitated him; in the higher sciences he had not one true disciple.

¹ The *Lyceum medicum Londinense*. See *Ottley's Life*, p. 85, and Chevalier's *Hunterian Oration*, 1821, p. 78.

How may this contrast be explained? Chiefly, I think, by the difference in the levels of men's minds in respect of the two subjects. There was as yet in this country no desire for comparative anatomy or physiology or any of the deeper objective studies of life. There was as yet no 'taste,' as we say, for these things, for none but Hunter had yet tasted in them the happiness of intellectual exercise.

In pathology it was very different. It had never been more actively studied than in Hunter's time. Practitioners of medicine and surgery were ready and able to receive his teaching; and though in his lifetime it hardly excited enthusiasm, yet the best of those who heard it saw that it showed the right way to knowledge, and that if their art were to be improved it must be by work on broader ground than ever before, and by men of larger higher culture.

In comparative anatomy and physiology Hunter was in advance of his time. Not far in advance, for Cuvier and Meckel quickly followed him; and then these sciences became, as they are now, the studies of some of the largest intellects. And it may well be, that if Hunter had been more apt to teach he would not have been without disciples. Some men by personal influence can make disciples even prematurely. They hasten the unready, attract or compel the unwilling. They are founders of schools before themselves are quite fit to be called scholars. Hunter was utterly void of the qualities by which such teachers make their schools. He had no attractions easy to be felt, no power of persuasion in speech or manner. His lectures were dull, tedious, ill-delivered. He was so busy in his search for knowledge, and so cautious in his estimate of it, that he always delayed to publish what he knew. It was only by the

sheer force of his work and example that he could have moved men to follow him. These were enough in surgery and medicine, not enough in the deeper physiology. And thus it was that when he died, poor and with work half done, there was scarcely one who knew how vast and various his labours in this field had been; there was not one who could complete his unfinished essays or the catalogues of his collection. The treasures of his museum and his manuscripts remained unknown for many years. His works had been like waves in advance of the on-coming tide. A few that watched them thought them grand and beautiful; but they broke on the shore in what seemed like only trouble and confusion, and the tide passed over them and hid the treasures they had borne.

It was not till Owen came that the treasures were recovered and worthily displayed. But by this time others had done much of the work of Hunter, and had reaped the just reward. Still it is of historic and personal interest to see in Owen's catalogues of the physiological portion of the museum, and more completely in his two volumes of Hunter's *Essays and Observations*, that Hunter had done vastly larger work in all the biological sciences than had appeared in what he published in his lifetime. That had been enough to prove him first among the biologists of his day; these showed more: they showed a yet wider range of study, and especially that, as Professor Flower has written, 'Hunter had collected materials for a work which needed but the finishing touches to have made it one of the greatest, most durable, and valuable contributions ever made by any one man to the advancement of the science of comparative anatomy.'¹

It may seem useless to dwell on these things and revive the vain regret that not Hunter alone, but

¹ Introductory Lecture, Feb. 14, 1870.

England, lost such honours ; but it is not useless to show the greatness and true character of the mind by which our English—I mean, of course, our national—school of surgery was founded ; for it is only by imitation, however distant, that the worth and honour of the school can be maintained.

Hunter's great renown is commonly told by saying that he was the founder of scientific surgery : and so he was ; for he first studied and taught, in the light and with the methods of a large knowledge of physiology, the very processes of disease and repair with which the practice of surgery is concerned. There were excellent surgeons before him and in his own time—sagacious, observant, practical men, by whose researches surgery was advancing rapidly in utility and precision. Not to speak of the best members of the French Academy, there were in Hunter's time at least three in this country, Cheselden, Percivall Pott, and Samuel Sharp. Of the first two Hunter was, for a short time, a pupil ; and they were all practical surgeons of the highest order, rich in experience, and teaching their experience in well-expressed rules. It is great praise of him to say that, as a practical surgeon, he was worthy to succeed them.

But all the surgery before the time of Hunter was only that of experience, and, in so far as it was a science at all, it stood alone. It was scarcely combined with medicine ; and it was not only a happy saying, but a recent doctrine which Abernethy expressed when, following Hunter, he said that medicine and surgery should be, with the title of the French republic, one and indivisible. To the sciences beyond medicine, surgery had no affinity at all. Surgery and physiology were far asunder ; no one strong mind had ever deeply studied both and become conscious that both were parts of the same science of

living things, and that each might give light to the other, and each be a test of the other's truth.

This was Hunter's greatest work in and for surgery. He brought the scientific method into the study of the practice, and he welded scientific knowledge with the lessons of experience. If we compare his works with those of Percivall Pott, the best of all his predecessors, the difference is clear. In practical surgery Pott generally appears more thoroughly instructed, a more 'complete surgeon'; but with the science and the exposition of principles, Hunter alone deals worthily.

And in all this Hunter was not only a thinker; he was a great worker. Any physiologist might have shown the utility of science for the improvement of the art of surgery; but, as I have said of Hunter's physiological work, so of this—with his own mind he planned and, as with his own hand, he did the work. He worked in scientific surgery with all the mental power, the variety of method, and the concentrated light that could be gained in the largest study of biology. He added to the knowledge of his time new facts and principles in surgery which it is literally impossible to count or over-value. His books are full of them.

To some readers of the surgical works of Hunter this may seem too high an estimate. Later researches, they may say, have found them very defective, sometimes erroneous, often insufficient, and unfit for place in modern systems. Forty years ago—that is nearly midway between Hunter's time and this—Mr. Palmer, the excellent editor of the best edition of the works, tried, by the addition of copious notes, to bring them into accordance with the pathology then accepted. We may now see with a kind of sad amusement that the notes need nearly as much amendment as the text; sometimes even more.

This is inevitable in science. This must be till the perfection of knowledge is attained. And yet that which once seemed true, and by men capable of judging was held to be true, may still command our admiration. For the body of science is like an organic world in process of evolution. At every time each living form is, in itself, complete, its parts are all in mutual fitness, and it is adapted to all the conditions in which it has to live. But every living form is transient; it passes while some higher form is being evolved from it, and this lives its time, and in its turn evolves some form yet higher and nearer to what we deem perfection.

It is so with the doctrines of science. As we look back in its history, we see grotesque forms of knowledge or belief, the extinct creations of observant, thoughtful men; and when we see how unfit they are to be associated with the forms and conditions of knowledge in our own time, we are apt to conclude that they were worse than incomplete; not merely shortcomings, but errors. Doubtless many were so; they were as species decaying and dying-out; deflections from the true route of progress. But many doctrines which now look like monstrous forms were, in their own time, very good; they fitted-in with the whole body of science then existent; they were as parts of an organic system, complete for its own time, potent for a better future. To those who possessed them they were right, even as to ourselves much of our knowledge is; though we cannot doubt that, a century or two hence, many of what now seem complete truths will be like extinct or monstrous forms, and of our very words few will remain in science with the same meanings as they bear now.

And in this is no disparagement of knowledge. That cannot be despicable which is, for the present, the

best possible, and has in itself capacity for change to a yet better future. But, similarly, that must not be despised which, in the past, was good, and though it fell short of the truth was not wholly erroneous from it. So, in estimating Hunter's contributions to the truths and laws of science, we must not scorn all those which have not borne unchanged the test of time; it would be as unreasonable to scorn a noble ancestry.

I have said much of Hunter's scientific surgery; and his great renown must rest on it. But I desire to correct an error into which, I think, some of his eulogists have fallen, when they have implied that his surgical practice was always based upon his scientific knowledge, and that no practice can be good unless it be deduced from physiology or pathology, or be, at least, consistent with what is held to be true in them.

Now, as to Hunter—and herein, again, he may be our model—he was very cautious in deductions. Few, I think, have known better than he did the danger of reasoning from physiology into practical surgery. As he wrote: 'The man who judges from general principles only, shows ignorance; few things are so simple as to come wholly within a general principle. We should never reason on general principles only, much less practise upon them, when we are or can be master of all the facts; but, when we have nothing else but the general principle, then we must take it for our guide.'¹

And his obedience to this wise rule is shown in the fact, that in many of his writings on surgical practice there is hardly a sign that he was a great physiologist. In his 'Treatise on the Venereal Disease' there is not a sentence that would plainly tell it; and even in his great, and chiefly physiological, work on 'Inflammation,' there is

¹ *Essays and Observations*, vol. i. p. 268.

no attempt to show that the right practice of surgery can be deduced from physiology. Even from pathology the practical deductions are very few. In all his practical writings we can detect the same scientific mind as was at work in physiology; the same care and laborious activity in collecting facts; the same quickness and usual accuracy in generalising; but the whole effort is to 'be master of all the facts,' and to learn and teach from them.

For one great example: it was by thus bringing the force of the scientific mind and method to bear on the facts of practical surgery, that Hunter achieved his great invention—that of curing aneurism by tying the artery far above the diseased part. This was not the result of any laborious physiological induction; it was mainly derived from facts very cautiously observed in the wards and dead-house. I shall not discuss Hunter's right to the honour of this invention: it is as clear as that of any discoverer of any fact in science. If any arbitration be needed between Anel and Desault on the one hand, and Hunter on the other, or any between France and England—for the question has been made of national interest—let the arbitrator be the Italian, Assalini. He had the singular good fortune to see three decisive operations. In 1781, he saw Spezzani tie the femoral artery in the sheath of the triceps muscle, preparatory to an intended amputation for a popliteal aneurism. In June 1785, he saw Desault, in Paris, tie the popliteal artery above a popliteal aneurism, which he did not lay open. In December of the same year he saw Hunter, at St. George's Hospital, for the same disease, tie the femoral artery in the canal of the triceps, and this operation, he says, 'excited the greatest wonder, and awakened the attention of all the surgeons of Europe.'¹

¹ Assalini, *Manuale di Chirurgia*, Parte Prima, p. 89.

It was really a splendid achievement, and its utility is not half told by counting the thousands of lives that it has saved. Its higher value is that it still abides as a great testimony of the power of the scientific mind in surgery. I think it has done more than any other of Hunter's works to make not only surgery but surgeons scientific. And herein is his greatest influence; for never in this country, since his time, has the study and teaching of practical surgery been divorced from the science of biology. The scientific spirit has never failed. It has been variously prevalent in different men and at different schools: but it has never failed; and even the most practical parts of surgery are now studied with such scientific method and completeness as before Hunter's time were never thought of.

We have still our distinctions of practical and scientific surgery, of the art and the science. And though the differences between them diminish every year, yet they will remain; and it is well they should do so, for they are suited to men of different tempers, tastes, or opportunities. Yes; both will remain, though Fergusson is gone: the greatest master of the art, the greatest practical surgeon of our time; and men can no longer watch the eyes that were so keen, or try to imitate the hands that were so strong and yet so sensitive and swift and light; or wonder at the ready and clear knowledge, the prompt invention, the perfect calmness in the midst of danger. These all are gone, and with them all that multiplied tenfold their charm—the warm heart, the friendliness, the generous rivalry, the social grace. These, too, are gone; but the memory of his lessons will remain with us; and, practical as he was, Fergusson would have taught that every surgeon should have, according to his ability, both art and science, and should work at both, as with two

hands. Either science or art alone can do many things ; even a one-handed man is far from helpless ; but the two hands are better ; and they should work together in harmony, with mutual help ; for the best work can be done only when the power and skill of science and art are combined as with one thought and one design. It was thus that Hunter wrought in surgery.

And mark, now, what he did for surgeons. Before his time they held a subordinate place in the medical profession. A few, with rare ability, had held good rank—as Wiseman, Cheselden, Hawkins, and Pott—but generally they were inferior to the physicians. And justly so ; for the physicians had not only better knowledge of their proper calling, but a far larger number of them were men of higher culture, well-educated gentlemen, and the associates of gentlemen. Besides, they were the chief teachers of all the medical sciences, the teachers even of anatomy to the surgeons. After the time of Hunter we may trace a well-marked change. Physicians worthily maintained their rank, as they do now ; but surgeons rose to it, and in the lessons of Hunter surgery gratefully repaid medicine for the teachings of a century. Following Hunter in the pursuit of science, surgeons soon became the chief anatomists, equal as physiologists and pathologists, and they gained entrance into the ranks of the most educated class. Yes ; Hunter did more than anyone to make us gentlemen. And the lesson of this fact is plain and emphatic, for it was not by force of social skill, by money, or any external advantage that he did this. From the few records that we have of him it is clear that he was a rough and simple-mannered man, abrupt and plain in speech, warm-hearted and sometimes rashly generous, emotional and impetuous, quickly moved to tears of sympathy, quickly ablaze with anger and fierce

words, never personally attractive, or seeming to have great mental powers, and always far too busy to think of influencing those around him. He had few friends, he gained the personal regard of very few, and no one paid him the homage of mimicry. The vast influence which he exercised on surgery and surgeons was the influence of the scientific mind. What follows? Surely, that if we desire to maintain the rank of gentlemen, to hold this highest prize of our profession, we must do so by the highest scientific culture to which we can attain. And to this we are bound, not for our own advancement alone, but by every motive of the plainest duty.

These are some of the grounds on which, beyond all dispute, John Hunter's memory deserves the honour that we pay to it to-day. And these are not nearly all; in want of time I have omitted many, in want of just appreciation more; and whatever one might now tell, one could not estimate his claim to future honour. The influence of such men as Hunter reaches far beyond the time and space of their conscious activity; their true thoughts live after them. Their true thoughts not only endure and remain: in the continuity of mental life they really live; they pass on from one generation to another, and in the minds of each succeeding generation they are developed, they grow, they attain more nearly to perfection.

Thus, when we honour the memory of Hunter, we honour not only that which is past, but that which is still present; a still abiding power doing good. For Hunter's true thoughts still live in us, and they will live after us, and never cease to help and urge men onward in the pursuit of truth. In the world of mind he that is mortal may produce that which may be immortal.

XI.

ON SOME OF THE SEQUELS OF TYPHOID FEVER.

Published in the second edition, 1879, of 'Clinical Lectures and Essays.'

I AM conscious of the danger which one who practises surgery usually incurs in writing on subjects that are naturally in the field of practical medicine. But among the diseases that are likely to follow typhoid fever there are some which occur in so advanced a period of the convalescence, and appear so local and so far dissociated from the fever, that they may be observed more often by surgeons than by physicians. I venture, therefore, to call attention to the frequent occurrence after typhoid fever of these following diseases, namely, phlebitis, periostitis with or without necrosis, especially periostitis of ribs, local paralysis of muscles.

Of the frequency of these diseases after typhoid fever I can have no doubt. I believe that I have seen, taking all together, more than a hundred cases in the last twelve years. I do not feel competent to deal with the question whether each fever has, as seems very probable, its own proper sequels, and is in this sense, though perhaps in less degree, as specific as in its fever-period; but, excepting the phlebitis, I have not seen any of the diseases I have enumerated after any other than typhoid fever, or a fever which I suppose to be closely related to it; and I have not seen after typhoid any corresponding number

of cases of large lymph-glands, diseased joints or ears, or other diseases of mere debility such as may follow any acute illness, and, notably, scarlet-fever.

The fever which I have referred to as 'closely related' to typhoid, is that which in the last few years has been prevalent in Naples, Rome, and I believe other places in middle and southern Italy. Some call it typhoid; some remittent; Neapolitans often call it Roman, Romans Neapolitan; in some cases I have ample reason to believe that it was genuine typhoid; and the likeness of its frequent sequels makes me ready to believe that it always was so.

It is common to all these sequels of typhoid fever that they appear when the patient is considered to be 'well of his fever,' when his temperature is normal and, I believe, constant; when he is beginning to move about and becoming stouter and stronger. I do not remember to have seen or heard of a case in which any of them occurred during the continuity of the fever.

The *phlebitis* has in it no character widely different from those in which it is seen in other of its so-called adhesive forms. Its most usual seat, by very far, is in the femoral, especially the left femoral, vein. It is generally marked by aching and tenderness along the line of the inflamed part of the vein, with stiffness at the groin, and more or less of firm general swelling of the limb. There is no marked access of fever, no other disturbance of the general health than may be ascribed to the local distress. The duration of the painful symptoms is, I think, quite uncertain; but after they have ceased some bigness and clumsiness of the limb usually remain for many months. In many cases the limb remains always larger than the other, but not weaker or less muscular; and in some, in which, I suppose, the

femoral vein is in more than the usual length obstructed, the superficial veins become very large and tortuous.

I have not seen or heard of a case in which suppuration attended the phlebitis; I have only heard of one in which embolism occurred; and I do not know of any in which the affected veins were examined after death. I believe that the condition is one of inflammatory swelling and thickening of the walls of the vein and consequent narrowing of its canal. Very probably, also, there is lymph-exudation or deposit in layers from the blood. I doubt whether thrombosis is ever a primary event, though it may occur as a consequence of the inflammatory changes in the lining of the vein.

The *periostitis* following typhoid fever has its most frequent seat on the tibiæ, but I have seen it on the femur, the ulna, the parietal bone. In one case it was symmetrical on the lower parts of the shafts of the tibiæ; in another it occurred on both tibiæ and one femur; but, with these exceptions, I have not seen it on more than one bone in the same person. It has always been circumscribed within a space of from one to three inches in extent, has always occurred in the convalescence, has never been attended with the delirium, fever, or other severe symptoms attendant on acute necrosis. At first it is only marked by the limited, hot and very tender swelling of the periosteum, and I have seen cases in which the periostitis has remained very long in this condition, and then slowly subsided without necrosis or any other abiding change of structure. In two cases, at least, the patient has remained for months—in one case for three years—subject to repeated attacks of pain and swelling of the periosteum, yet without signs of suppuration or more change than a slight permanent thickening. In one case a recurrence of periostitis ensued

after nine years of apparent soundness. In two cases the suppuration was not between the periosteum and bone, but in the substance of the periosteum, and in both these it has continued for many months with serious impairment of health.

When the periostitis has been associated with necrosis, the extent of the dead bone has always been far less than that of the inflammation over it, and, with rare exceptions, only the hard compact structure of wall, or the outer table, has perished. The separation of the dead bone has taken place in the usual way, and has been followed by healing which, so far as I have seen, has been permanent.¹

The *periostitis of the ribs* is in so much unlike the kind just described, that I have never yet seen it associated with necrosis. It so nearly resembles the ordinary scrofulous periostitis of ribs, that I have sometimes thought it should be regarded as only an evidence of scrofula educed by the feebleness of nutrition consequent on the fever. Yet I have never seen it associated with other similar signs of scrofula, and it has occurred, after typhoid, in some of so robust and apparently unblemished constitution that it would seem absurd to impute scrofula to them.

In the usual course of this disease, beginning always, I believe, in the convalescence, a well-defined round or oval swelling is found on one or more ribs. Whether on bone or cartilage, the swelling is generally on the front of the chest. It is painful and tender, and suppuration slowly ensues in it. Then slowly it discharges

¹ Cases narrated by Dr. Poignard make me believe that the laryngeal cartilages are apt to suffer necrosis in the convalescence of typhoid. See *Medical Times and Gazette*, November 30, 1878, p. 644: from the *Recueil de Méd. Militaire*, August.

itself through one or more channels leading obliquely through the overlying structures and through small openings in the skin. The flow of pus, thin and pale, continues usually for many months; I have known it continue for even two years; and not rarely fresh openings are formed for branches from the first channel of discharge. In the end complete healing takes place, with deep depressed scars and complete clearing-up of all the periosteal swelling.

I have never known necrosis of a rib in these cases, nor ever seen or heard of any advantage from laying wide open any of the sinuses. The general health never appears to be seriously affected by the abscesses, unless in persons of naturally feeble health. Commonly, indeed, the patients feel quite well, and can be active, suffering nothing more than slight local inconvenience.

All the cases of this sequel of typhoid fever that I have seen have been singularly alike, varying only in the position of the abscess. In one, in which I believe that the disease began in the periosteum of the eleventh or twelfth rib, a great abscess slowly made its way between the abdominal muscles, and I had to open it at the groin. In that case alone have I seen life endangered; but it ended well.

Local paralysis of muscles in the convalescence of typhoid I have seen in only a few cases, and in these the muscles affected were those supplied by the peroneal or the anterior tibial nerve. I have seen intense neuralgia with spasm imitating, in one case, disease of the hip-joint, in another, disease of the spine; and twice I have seen paraplegia. I am assured, also, of one instance of five members of a family having more or less extended paralysis after typhoid or typhus fever. But far better knowledge than I can supply of

the paralysis following typhoid may be found in a paper by Dr. Nothnagel.¹ He has seen or found on record cases not only of local motor paralysis, but of hemiplegia, paraplegia, some muscular tremors, and spasms, anæsthesia, hyperæsthesia, and neuralgia.

I may refer to his paper for its value in regard not only to these sequels of typhoid fever but to the general pathology of convalescence. I remember that the late Dr. Baly used to say that a book on the diseases of convalescence might be one of the most valuable that any physician could write. Perhaps I may hope that to such an one I have now contributed; but a far larger and more important contribution has been made by Dr. Keen of Philadelphia in a lecture 'On the Surgical Complications and Sequels of the Continued Fevers,' and in the copious bibliography appended to it.²

¹ Die nervösen Nachkrankheiten des Abdominaltyphus, in the *Deutsches Archiv für Klinische Medicin*, Bd. ix. p. 480, 1872.

² Published at Washington, 1877.

XII.

ANÆSTHETICS: THE HISTORY OF A DISCOVERY.

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December, 1879.*

THE history of the discovery of methods for the prevention of pain in surgical operations deserves to be considered by all who study either the means by which knowledge is advanced or the lives of those by whom beneficial discoveries are made. And this history may best be traced in the events which led to and followed the use of nitrous oxide gas, of sulphuric ether, and of chloroform as anæsthetics—that is, as means by which complete insensibility may be safely produced and so long maintained that a surgical operation, of whatever severity and however prolonged, may be absolutely painless.

In 1798, Mr. Humphry Davy, an apprentice to Mr. Borlase, a surgeon at Bodmin, had so distinguished himself by zeal and power in the study of chemistry and natural philosophy, that he was invited by Dr. Beddoes, of Bristol, to become the 'superintendent of the Pneumatic Institution which had been established at Clifton for the purpose of trying the medicinal effects of different gases.' He obtained release from his apprenticeship, accepted the appointment, and devoted himself to the study of gases, not only in their medicinal effects, but

much more in all their chemical and physical relations. After two years' work he published his *Researches, Chemical and Philosophical, chiefly concerning Nitrous Oxide*, an essay proving a truly marvellous ingenuity, patience, and courage in experiments, and such a power of observing and of thinking as has rarely if ever been surpassed by any scientific man of Davy's age; for he was then only twenty-two.

In his inhalations of the nitrous oxide gas he observed all the phenomena of mental excitement, of exalted imagination, enthusiasm, merriment, restlessness, from which it gained its popular name of 'laughing gas'; and he saw people made, at least for some short time and in some measure, insensible by it. So, among other suggestions or guesses about probable medicinal uses of inhalation of gases, he wrote, near the end of his essay: 'As nitrous oxide in its extensive operation appears capable of destroying physical pain, it may probably be used with advantage during surgical operations in which no great effusion of blood takes place.'

It seems strange that no one caught at a suggestion such as this. True, the evidence on which it was founded was very slight; it was with a rare scientific power that Davy had thought out so far beyond his facts; but he had thought clearly, and as clearly told his belief. Yet no one earnestly regarded it. The nitrous oxide might have been of as little general interest as the carbonic or any other, had it not been for the strange and various excitements produced by its inhalation. These made it a favourite subject with chemical lecturers, and year after year, in nearly every chemical theatre, it was fun to inhale it after the lecture on the gaseous compounds of nitrogen; and among those who inhaled it there must have been many who, in their intoxication, received sharp

and heavy blows, but, at the time, felt no pain. And this went on for more than forty years, exciting nothing worthy to be called thought or observation, till, in December 1844, Mr. Colton, a popular itinerant lecturer on chemistry, delivered a lecture on 'laughing gas' in Hartford, Connecticut. Among his auditors was Mr. Horace Wells, an enterprising dentist in that town, a man of some power in mechanical invention. After the lecture came the usual amusement of inhaling the gas, and Wells, in whom long wishing had bred a kind of belief that something might be found to make tooth-drawing painless, observed that one of the men excited by the gas was not conscious of hurting himself when he fell on the benches and bruised and cut his knees. Even when he became calm and clear-headed the man was sure that he did not feel pain at the time of his fall. Wells was at once convinced—more easily convinced than a man of more scientific mind would have been—that, during similar insensibility, teeth might be drawn without pain, and he determined that himself and one of his own largest teeth should be the first for trial. Next morning Colton gave him the gas, and his friend Dr. Riggs extracted his tooth. He remained unconscious for a few moments, and then exclaimed, 'A new era in tooth-pulling! It did not hurt me more than the prick of a pin. It is the greatest discovery ever made.'

In the next three weeks Wells extracted teeth from some twelve or fifteen persons under the influence of the nitrous oxide, and gave pain to only two or three. Dr. Riggs, also, used it with the same success, and the practice was well known and talked of in Hartford.

Encouraged by his success Wells went to Boston, wishing to enlarge the reputation of his discovery and to have an opportunity of giving the gas to some one under-

going a surgical operation. Dr. J. C. Warren, the senior Surgeon of the Massachusetts General Hospital, to whom he applied for this purpose, asked him to show first its effects on some one from whom he would draw a tooth. He undertook to do this in the theatre of the medical college before a large class of students, to whom he had, on a previous day, explained his plan. Unluckily, the bag of gas from which the patient was inhaling was taken away too soon; he cried out when his tooth was drawn; the students hissed and hooted; and the discovery was denounced as an imposture.

Wells left Boston disappointed and disheartened; he fell ill, and was for many months unable to practise his profession. Soon afterwards he gave up dentistry, and neglected the use and study of the nitrous oxide, till he was recalled to it by a discovery even more important than his own.

The thread of the history of nitrous oxide may be broken here.

The inhalation of sulphuric ether was often, even in the last century, used for the relief of spasmodic asthma, phthisis, and some other diseases of the chest. Dr. Beddoes and others thus wrote of it: but its utility was not great, and there is no evidence that this use of it had any influence on the discovery of its higher value, unless it were, very indirectly, in its having led to its being found useful for soothing the irritation produced by inhaling chlorine. Much more was due to its being used, like nitrous oxide, for the fun of the excitement which its diluted vapour would produce in those who freely inhaled it.

The beginning of its use for this purpose is not clear. In the *Journal of Science and the Arts*, published in 1818 at the Royal Institution, there is a short

anonymous statement among the 'Miscellanea,' in which it is said, 'When the vapour of ether mixed with common air is inhaled, it produces effects very similar to those occasioned by nitrous oxide.' The method of inhaling and its effects are described, and then 'it is necessary to use caution in making experiments of this kind. By the imprudent inspiration of ether a gentleman was thrown into a very lethargic state, which continued with occasional periods of intermission for more than thirty hours, and a great depression of spirits; for many days the pulse was so much lowered that considerable fears were entertained for his life.'

The statement of these facts has been ascribed to Faraday, under whose management the journal was at that time published. But, whoever wrote or whoever may have read the statement, it was, for all useful purposes, as much neglected as was Davy's suggestion of the utility of the nitrous oxide. The last sentence, quoted as it was by Pereira and others writing on the uses of ether, excited much more fear of death than hope of ease from ether-inhalation. Such effects as are described in it are of exceeding rarity; their danger was greatly over-estimated; but the account of them was enough to discourage all useful research.

But, as the sulphuric ether would 'produce effects very similar to those occasioned by nitrous oxide,' and was much the more easy to procure, it came to be often inhaled, for amusement, by chemists' lads and by pupils in the dispensaries of surgeons. It was often thus used by young people in many places in the United States. They had what they called 'ether-frolics,' in which they inhaled ether till they became merry, or in some other way absurdly excited or, sometimes, completely insensible.

Among those who had joined in these ether-frolics

was Dr. Wilhite, of Anderson, South Carolina. In one of them, in 1839, when nearly all of the party had been inhaling and some had been laughing, some crying, some fighting—just as they might have done if they had had the nitrous oxide gas—Wilhite, then a lad of seventeen, saw a negro-boy at the door and tried to persuade him to inhale. He refused and resisted all attempts to make him do it, till they seized him, held him down, and kept a handkerchief wet with ether close over his mouth. Presently his struggles ceased ; he lay insensible, snoring, past all arousing ; he seemed to be dying. And thus he lay for an hour, till medical help came and, with shaking, slapping, and cold splashing, he was awakened and suffered no harm.

The fright at having, it was supposed, so nearly killed the boy, put an end to ether-frolics in that neighbourhood ; but in 1842, Wilhite had become a pupil of Dr. Crauford Long, practising at that time at Jefferson (Jackson County, Georgia). Here he and Dr. Long and three fellow-pupils often amused themselves with the ether inhalation, and Dr. Long observed that when he became furiously excited, as he often did, he was unconscious of the blows which he, by chance, received as he rushed or tumbled about. He observed the same in his pupils ; and thinking over this, and emboldened by what Mr. Wilhite told him of the negro-boy recovering after an hour's insensibility, he determined to try whether the ether-inhalation would make any one insensible of the pain of an operation. So, in March 1842, nearly three years before Wells's observations with the nitrous oxide, he induced a Mr. Venable, who had been very fond of inhaling ether, to inhale it till he was quite insensible. Then he dissected a tumour from his neck ; no pain was felt, and no harm followed. Three months later, he

similarly removed another tumour from him ; and again, in 1842 and in 1845, he operated on other three patients, and none felt pain. His operations were known and talked of in his neighbourhood ; but the neighbourhood was only that of an obscure little town ; and he did not publish any of his observations. The record of his first operation was only entered in his ledger :

‘ James Venable, 1842. Ether and excising tumour, \$2.00.’ He waited to test the ether more thoroughly in some greater operation than those in which he had yet tried it ; and then he would have published his account of it. While he was waiting, others began to stir more actively in busier places, where his work was quite unknown, not even heard of.

Among those with whom, in his unlucky visit to Boston, Wells talked of his use of the nitrous oxide, and of the great discovery which he believed that he had made, were Dr. Morton and Dr. Charles Jackson, men widely different in character and pursuit, but inseparable in the next chapter of the history of anæsthetics.

Morton was a restless energetic dentist, a rough man, resolute to get practice and make his fortune. Jackson was a quiet scientific gentleman, unpractical and unselfish, in good repute as a chemist, geologist, and mineralogist. At the time of Wells’s visit, Morton, who had been his pupil in 1842, and for a short time, in 1843, his partner, was studying medicine and anatomy at the Massachusetts Medical College, and was living in Jackson’s house. Neither Morton nor Jackson put much if any faith in Wells’s story, and Morton witnessed his failure in the medical theatre. Still, Morton had it in his head that tooth-drawing might somehow be made painless, and even after Wells had retired from practice, he talked with him about it, and made some experiments, but, having no

scientific skill or knowledge, they led to nothing. Still, he would not rest, and he was guided to success by Jackson, whom Wells advised him to ask to make some nitrous oxide gas for him.

Jackson had long known, as many others did, of sulphuric ether being inhaled for amusement, and of its producing effects like those of nitrous oxide: he knew also of its employment as a remedy for the irritation caused by inhaling chlorine. He had himself used it for this purpose, and once, in 1842, while using it, he became completely insensible. He had thus been led to think that the pure ether might be used for the prevention of pain in surgical operations; he spoke of it with some scientific friends, and sometimes advised a trial of it; but he did not urge it or take any active steps to promote even the trial. One evening, Morton, who was now in practice as a dentist, called on him, full of some scheme which he did not divulge, and urgent for success in painless tooth-drawing. Jackson advised him to use the ether, and taught him how to use it.

On that same evening, the 30th of September, 1846, Morton inhaled the ether, put himself to sleep, and, when he awoke, found that he had been asleep for eight minutes. Instantly, as he tells, he looked for an opportunity of giving it to a patient; and one just then coming in, a stout healthy man, he induced him to inhale, made him quite insensible, and drew his tooth without his having the least consciousness of what was done.

But the great step had yet to be made—the step which Wells would have tried to make if his test-experiment had not failed. Clearly, operations as swift as that of tooth-drawing might be rendered painless, but could it be right to incur the risk of insensibility long enough and deep enough for a large surgical operation? It was

generally believed that in such insensibility there was serious danger to life. Was it really so? Jackson advised Morton to ask Dr. J. C. Warren to let him try, and Warren dared to let him. It is hard, now, to think how bold the enterprise must have seemed to those who were capable of thinking accurately on the facts then known.

The first trial was made on the 16th of October, 1846. Morton gave the ether to a patient in the Massachusetts General Hospital, and Dr. Warren removed a tumour from his neck. The result was not complete success; the patient hardly felt the pain of the cutting, but he was aware that the operation was being performed. On the next day, in a severer operation by Dr. Hayward, the success was perfect; the patient felt nothing, and in long insensibility there was no appearance of danger to life.

The discovery might already be deemed complete; for the trials of the next following days had the same success, and thence onwards the use of the ether extended over constantly widening fields. A coarse but feeble opposition was raised by some American dentists; a few surgeons were over-cautious in their warnings against suspected dangers; a few maintained that pain was very useful, necessary perhaps to sound healing; some were hindered by their dislike of the patent which Morton and Jackson took out; but as fast as the news could be carried from one continent to another, and from town to town, so fast did the use of ether spread. It might almost be said that in every place, at least in Europe, where the discovery was promoted more quickly than in America, the month might be named before which all operative surgery was agonising, and after which it was painless.

But there were other great pains yet to be prevented,

the pains of childbirth. For escape from these the honour and deep gratitude are due to Sir James Simpson. No energy, or knowledge, or power of language less than his could have overcome the fears that the insensibility, which was proved to be harmless in surgical operations and their consequences, should be often fatal or very mischievous in parturition. And to these fears were added a crowd of pious protests (raised, for the most part, by men) against so gross an interference as this seemed with the ordained course of human nature. Simpson, with equal force of words and work, beat all down ; and by his adoption of chloroform as a substitute for ether promoted the whole use of anæsthetics.

Ether and chloroform seemed to supply all that could be wished from anæsthetics. The range of their utility extended ; the only question was as to their respective advantages, a question still unsettled. Their potency was found absolute, their safety very nearly complete, and, after the death of Wells in 1848, nitrous oxide was soon neglected and almost forgotten. Thus it remained till 1862, nearly seventeen years, when Mr. Colton, who still continued lecturing and giving the gas 'for fun,' was at New Haven, Connecticut. He had often told what Wells had done with nitrous oxide at Hartford, and he wanted other dentists to use it, but none seemed to care for it till, at New Britain, Dr. Dunham asked him to give it to a patient to whom it was thought the ether might be dangerous. The result was excellent, and in 1863, Dr. Smith of New Haven substituted the nitrous oxide for ether in his practice and used it very frequently. In the nine months following his first use of it, he extracted without pain nearly 4,000 teeth. Colton, in

the following year, associated himself with a dentist in New York and established the Colton Dental Association, where the gas was given to many thousands more. Still, its use was very slowly admitted. Some called it dangerous, others were content with chloroform and ether, others said that the short pangs of tooth-drawing had better be endured. But in 1867 Mr. Colton came to Paris and Dr. Evans at once promoted his plan. In 1868 he came to London and, after careful study of it at the Dental Hospital, the nitrous oxide was speedily adopted, both by dentists and by the administrators of anæsthetics. By this time it has saved hundreds of thousands of people from the sharp pains of all kinds of operations on the teeth and of a great number of the surgical operations that can be quickly done.

Such is the history of the discovery of the use of anæsthetics. Probably, none has ever added so largely to that part of happiness which consists in the escape from pain. Past all counting is the sum of happiness enjoyed by the millions who in the last three-and-thirty years have escaped the pains that were inevitable in surgical operations; pains made more terrible by apprehension, more keen by close attention; sometimes awful in a swift agony, sometimes prolonged beyond even the most patient endurance, and then renewed in memory and terrible in dreams. These will never be felt again. But the value of the discovery is not limited by the abolition of these pains or the pains of childbirth. It would need a long essay to tell how it has enlarged the field of useful surgery, making many things easy that were difficult, many safe that were too perilous, many practicable that were nearly impossible. And, yet more variously, the discovery has brought happiness in the relief of some of the intensest pains of sickness, in quieting convulsion, in helping to the

discrimination of obscure diseases. The tale of its utility would not end here ; another essay might tell its multi-form uses in the study of physiology, reaching even to that of the elemental processes in plants, for these, as Claude Bernard has shown, may be completely for a time suspended in the sleep produced by chloroform or ether.

And now, what of the discoverers ?¹ What did time bring to those who brought so great happiness to mankind ?

Long, out-stripped in the race of discovery, continued in his quiet useful life in practice at Jefferson, and after 1850 at Athens (Georgia). The fact of his discovery was not known beyond his own narrow circle of friends till the use of both ether and chloroform had become general. Then his claim to honour was as little heard as one gentle voice might be in the uproar of a confused and noisy crowd. In 1853, when Morton made one of his claims on Congress for a large reward in money, it was shown that Long had used the ether more than four years before him. The claim to honour could not be denied. It was

¹ Those only are here reckoned as discoverers from whose work may be traced not merely what might have been the beginning of the discovery, but the continuous history of events consequent on the evidence of its truth. Long, it is true, might under this rule be excluded ; yet his work cannot fairly be separated from the history. Of course in this, as in every similar case, there were some who maintained that there was nothing new in it. Before 1842 there were many instances in which persons underwent operations during insensibility. There may be very reasonable doubts about what is told of the ancient uses of Indian hemp and mandragora ; but most of those who saw much surgery before 1846 must have seen operations done on patients during insensibility produced by narcotics, dead-drunkenness, mesmerism, large losses of blood, or other uncertain and often impracticable methods. Besides, there were many guesses and suggestions for making operations painless. But they were all fruitless ; and they fail at that which may be a fair test for most of the claims of discoverers—the test of consequent and continuous history. When honour is claimed for the authors of such fruitless works as these, it may fairly be said that blame rather than praise is due to them. Having seen so far as they profess, they should not have rested till they could see much further.

admitted by Jackson, who wrote that if Long 'had written to him in season,' he 'would have presented his claims to the Academy of Sciences of France.' But nothing followed; and Long's name and worth were known to very few till, in 1877, Dr. Marion Sims published a full account of his observations. Long was then an old man, still at work in his profession and obliged to work very hard, for he had suffered heavy losses in the Civil War. He had the esteem of all who knew him; he was, as Jackson described him, 'a very modest, retiring man, an honourable man in all respects.' Last year he died, and then he was honoured by his portrait being presented by Mr. Stuart of New York to the Alumni of the University of Georgia and placed in the Capitol of that State.

Of Wells, it has already been told how, after his failure in the attempt to show the value of nitrous oxide at Boston, he went home disheartened, and was long ill and unable to practise his profession. He gave up his dentistry and occupied himself in many things, the last of them picture-dealing, and he left the study of the nitrous oxide till after the full discovery of the use of ether. Then he tried in vain to prove that his method of anæsthesia was the best and safest, not in dentistry alone, but in severer surgery; he and Dr. Marcy, he said, had used ether before Morton did, and found it in no way better than the nitrous oxide. But it was in vain that he tried to gain honour or reward for priority of discovery, or to recover the position and the practice he had given up. Constant disappointment weighed heavily on him; for he was a gentle, sensitive, enthusiastic man. At last, weary and wretched, he became insane, and in 1848 committed suicide. Some twenty years afterwards, the nitrous oxide was in full use; his statue was set up in Hartford; and

five years later, his widow, still in poverty, was helped by a subscription.

Morton, as vainly, but with longer contest, strove to 'make his fortune.' As soon as the value of the ether-inhalation was proved, he took out a patent for its use, and Jackson was induced to join him in this wrong. But at once there arose a fierce, coarse controversy as to which of the two should have the honour of priority of discovery, and what should be their several profits from the patent. The principals did not write so much as their friends and their attorneys; these were vehement and profuse, and the dispute was made yet more coarse and bitter by those who acquired money-interest in the patent, and by the editors of newspapers in which Morton advertised for practice and for the sale of licences to use his patent. One cannot read the controversy without utter shame at the degradation of truths which should have been told only in the pure language of science and humanity. Some of it is so written; but more is foul with conflicting affidavits, special pleadings of lawyers, perversions of low journalists and mutual charges of falsehood, fraud, and imposition. Of course, no good came of it: Jackson retired from it as soon as he could, and Morton found his patent useless. He and his friends often spoke of the ether as a new compound, and some called it Letheon; and he set up a Letheon-establishment, but every one knew that the 'compound' was sulphuric ether. Jackson never attempted to conceal it, and there was no need of apparatus in its use. The patent cost so much more than it yielded that in a year or two Morton was a very poor man, needing money-help from his friends. A thousand dollars were subscribed for him, and then there was a bitter quarrel as to whether they were given for honour or for charity.

Many times he petitioned Congress for some large

reward ; he spent time and money in 'lobbying,' and the worse means of gaining votes, but all was in vain. His petitions were met with protests from Jackson, with claims for Wells's family, or for Long, and every discussion raised the old controversy, and made it hotter with the heat of new personal and political animosities ; for the South was then dominant and Morton was a Northerner. At the end of every attempt to get money he found himself rather poorer, in less practice, and in worse repute ; people became utterly tired of the quarrel and, for the most part, indifferent to the claims of all who were engaged in it. Even the discovery itself was less esteemed in America than in Europe, so that that could be often repeated which Dr. Hayward had said of it : ' The only spot in Christendom in which the discovery was received with coldness was in our own country.'

Thus Morton worried his way through life for two-and-twenty years. His friends helped him from time to time with money and encouragement, but at last even his rough hard nature yielded. After one of his many failures to obtain money from Congress, driven half wild by poverty and disappointment, he returned from Washington to New York in a state of intense mental excitement. He was kindly tended by Dr. Lewis Sayre and carefully watched. One day, in July 1868, he drove out with his attendant, wanting, as he said, fresh air 'to cool his brain.' Returning home he suddenly jumped from the carriage, ran through a plantation to the border of an adjacent lake, put his head into the water 'to cool his brain,' was found insensible, and in a few hours died.

Jackson found himself in trouble as soon as his belief that ether might be used to produce insensibility in surgical operations was proved true by Morton. He seems to have had no selfish view beyond that of main-

taining his claim to the honour of the discovery ; but to maintain this, he was involved in the discredit of the patent, and in all the controversies had to endure coarse abuse and the imputation of trickery and low motives. He separated himself from Morton as soon as he could, took as little part as possible in the controversy, and all that he wrote was gentle and courteous in comparison with the language of his opponents. He held on his course in the study of mineralogy and geology, wrote many good papers on them, was well esteemed in all the scientific societies of his country, and received scientific honours from abroad. But some years ago his intellectual power failed, and now he is in an asylum, without either wealth or honours, but happy in the peaceful enjoyment of genial and benevolent delusions.

Simpson had a more prosperous career than these. His introduction of the use of anæsthetics into obstetric practice, and the generally wider range for their employment which was due to his adoption of chloroform in place of ether, were part of the merits which gained for him, among many other honours, a baronetcy and a place among the most successful physicians of his time, a statue in Edinburgh and a memorial bust in Westminster Abbey.

It is hard to repress a first feeling of shame and anger that the American discoverers, great benefactors as they were, should have been left by their fellow-countrymen to die poor, without honour, scarcely thanked for their work. The whole world owes to them immeasurable happiness. America owes them, besides, the honour of a great national renown. Where, then, was the bounty which, in that country, in so many instances, has been splendid in its grandeur ? Where the keen jealousy for national honour ?

It looks as if all had fallen into some sullen ingratitude and indifference.

But, if we look more deeply, we may find no reason for blaming the American people; rather, we may find that they did only what, in the like conditions, would have been done by ourselves or any others. The case was one in which it was made necessary to satisfy, if possible, both gratitude and justice. This might have been easy if there had been only one claimant; gratitude might have been profuse and bountiful, and justice might have approved or, at least, been silent. But there were never less than two, and generally four, claimants; and where gratitude might with a free hand have been ready to give honours and rewards to them all, justice was invoked that the gifts might be in measure proportioned to their several merits. Then gratitude, waiting on justice, became irresolute and cold, or was distracted by new objects.

It may seem very hard; but let any one or, much better, let any four or five, as if sitting in council, think what they would have done; how they would have satisfied at once their gratitude and their sense of justice; how much, suppose, of any great vote by Congress they would have given to each claimant.

How much should Long have had? He first used a true, safe, and sufficient anæsthetic in surgery, and used it with such success that, if he had quickly published his facts, he could not but have been regarded as the great discoverer. It was the fault of his position more than of himself that his facts were not sooner known; and for his delay he might, in so grave a case, plead prudence. But was he then to have no reward?

And what should have been Wells's share? He certainly discovered the use of nitrous oxide, and from his success with it may be traced, not only the knowledge

of its whole present utility, but the continuous history of the complete discovery of anæsthetics. True, he soon left the field, disheartened and as if in distrust of his own work ; but before he left it he had set Morton on the track, and had thus contributed to the discovery of the uses of ether and chloroform. These, surely, were great merits ; what should have been their reward ?

Jackson's claims were of a different kind. He had what may be called a scientific idea of the anæsthetic use of ether ; but he gave it no active life, no clear persuasive expression. His mind was chiefly occupied in fields of science far apart from active surgery ; the great idea needed transplantation. But, when we see to what it grew, we must admit that he who bred and nurtured it, and then gave it to be planted, had great claims to honour.

Morton answered well to the definition given, it is said, by Sydney Smith : 'He is not the inventor who first says the thing, but he who says it so long, loudly, and clearly, that he compels mankind to hear him.' Without either skill, or knowledge, or ingenuity, he supplied the qualities without which the complete discovery of anæsthetics might have been, at least, long delayed—boldness, perseverance, self-confidence. While Long waited, and Wells turned back, and Jackson was thinking, and those to whom they had talked were neither acting nor thinking, Morton, the 'practical man,' went to work and worked resolutely. He gave ether successfully in severe surgical operations, he loudly proclaimed his deeds, and 'compelled mankind to hear him.' His claim was very clear.

Probably, most people would agree that all four deserved reward ; but that which the controversy and the patent and the employment of legal advisers made

it necessary to determine was, whether more than one deserved reward and, if more than one, the proportion to be assigned to each. Here was the difficulty. The French Academy of Sciences in 1850 granted equal shares in the Monthyon Prize to Jackson and to Morton; but Long was unknown to them, and, at the time of the award, the value of nitrous oxide was so hidden by the greater value of ether that Wells's claim was set aside. A memorial column was erected at Boston, soon after Morton's death in 1868, and here the difficulty was shirked by dedicating the column to the discovery of ether, and not naming the discoverers. The difficulty could not be thus settled; and, in all probability, our supposed council of four or five would not solve it. One would prefer the claims of absolute priority; another those of suggestive science; another the courage of bold adventure; sentiment and sympathy would variously affect their judgments. And if we suppose that they, like the American Congress, had to discuss their differences within sound of such controversies as followed Morton's first use of ether, or during a war of pamphlets, or under burdens of parliamentary papers, we should expect that their clearest decision would be that a just decision could not be given, and that gratitude must die if it had to wait till distributive justice could be satisfied. The gloomy fate of the American discoverers makes one wish that gratitude could have been let flow of its own impulse; it would have done less wrong than the desire for justice did. A lesson of the whole story is that gratitude and justice are often incompatible; and that when they conflict, then, usually, *summum jus summa injuria*.

Another lesson, which has been taught in the history of many other discoveries, is clear in this—the lesson that

great truths may be very near us and yet be not discerned. Of course, the way to the discovery of anæsthetics was much more difficult than it now seems. It was very difficult to produce complete insensibility with nitrous oxide till it could be given undiluted and unmixed; this required much better apparatus than Davy or Wells had; and it was hardly possible to make such apparatus till indiarubber manufactures were improved. It was very difficult to believe that profound and long insensibility could be safe, or that the appearances of impending death were altogether fallacious. Bold as Davy was, bold even to recklessness in his experiments on himself, he would not have ventured to produce deliberately in any one a state so like a final suffocation as we now look at unmoved. It was a boldness not of knowledge that first made light of such signs of dying, and found that what looked like a sleep of death was as safe as the beginning of a night's rest. Still, with all fair allowance for these and other difficulties, we cannot but see and wonder that for more than forty years of this century a great truth lay unobserved, though it was covered with only so thin a veil that a careful physiological research must have discovered it. The discovery ought to have been made by following the suggestion of Davy. The book in which he wrote that 'nitrous oxide—capable of destroying physical pain—may probably be used with advantage during surgical operations,' was widely read, and it would be hard to name a man of science more widely known and talked of than he was. Within two years of the publication of his *Researches*, he was appointed to a professorship in the Royal Institution; and in the next year he was a favourite in the fashionable as well as in the scientific world; and all his life through he was intimately associated with those among whom all the

various motives for desiring to find some means 'capable of destroying physical pain' would be most strongly felt. Curiosity, the love of truth, the love of marvels, the desire of ease, self-interest, benevolence—all were alert in the minds of men and women who knew and trusted whatever Davy said or wrote, but not one mind was earnestly directed to the rare promise which his words contained. His own mind was turned with its full force to other studies; the interest in surgery which he may have felt during his apprenticeship at Bodmin was lost in his devotion to poetry, philosophy, and natural science, and there is no evidence that he urged others to undertake the study which he left. Even his biographers, his brother Dr. John Davy and his intimate friend Dr. Paris, both of whom were very capable physicians and men of active intellect, say nothing of his suggestion of the use of nitrous oxide. It was overlooked and utterly forgotten till the prophecy was fulfilled by those who had never heard of it. The same may be said of what Faraday, if it were he, wrote of the influence of sulphuric ether. All was soon forgotten, and the clue to the discovery, which would have been far easier with ether than with nitrous oxide, for it needed no apparatus, and even required mixture with air, was again lost. One could have wished that the honour of bringing so great a boon to men, and so great a help in the pursuit of knowledge, had been won by some of those who were giving themselves with careful cultivation to the search for truth as for its own sake. But it was not so: science was utterly at fault; and it was shown that in the search for truth there are contingencies in which men of ready belief and rough enterprise, seeking for mere utility even with selfish purposes, can achieve more than those who restrain themselves within the range of what seems reasonable.

Such instances of delay in the discovery of truth are always wondered at, but they are not uncommon. Long before Jenner demonstrated the utility of vaccination it was known in Gloucestershire that they who had had cow-pox could not catch the small-pox. For some years before the invention of electric telegraphy, Professor Cumming of Cambridge, when describing to his class the then recent discovery by Oersted of the power of an electric current to deflect a magnet, used to say, 'Here, then, are the elements which would excellently serve for a system of telegraphy.' Yet none of his hearers, active and cultivated as they were, were moved from the routine of study. Laennec quotes a sentence from Hippocrates which, if it had been carefully studied, might have led to the full discovery of auscultation. Thus it often has been; and few prophecies can be safer than that our successors will wonder at us as we do at those before us; will wonder that we did not discern the great truths which they will say were all around us, within reach of any clear, earnest mind.

They will wonder, too, as we may, when we study the history of the discovery of anæsthetics, at the quietude with which habitual miseries are borne; at the very faint impulse to action which is given by even great necessities when they are habitual. Thinking of the pain of surgical operations, one would think that men would have rushed after the barest chance of putting an end to it as they would have rushed to escape from starving. But it was not so; the misery was so frequent, so nearly customary, deemed so inevitable, that, though it excited horror when it was talked of, it did not excite to strenuous action. Remedies were wished for and sometimes tried, but all was done vaguely and faintly; there was neither hope enough to excite intense desire, nor desire enough to encourage hope; the misery was 'put up with' just as we now put

up with typhoid fever and sea-sickness, with local floods and droughts, with the waste of health and wealth in the pollutions of rivers, with hideous noises and foul smells, and many other miseries. Our successors, when they have remedied or prevented them, will look back on them with horror, and on us with wonder and contempt for what they will call our idleness or blindness or indifference to suffering.

XIII.

ELEMENTAL PATHOLOGY.

Presidential Address to the Pathological Section, at the Cambridge meeting of the British Medical Association, 1880.

EVERY pathologist must have felt that the greatest difficulty in his study is in the manifold complexity of the body in which it is pursued. The living human body is, surely, the most complex mass of matter in the known world. In composition, it surpasses the highest powers of chemical analysis; in mechanism, it is as far beyond the calculations of the physicist; its structures are but dimly seen with even the most perfect microscope; all the known forces of nature are constantly and coincidently at work within it; through circulating blood and a nervous system every part is within swift communication with all the rest; and it includes the apparatus of a mind from whose influence no portion of its matter is distantly removed. And in this body the pathologist has to study, not that which is fixed, orderly, and natural, but that which is in disorder and unsettled. May we not, therefore, hold that, among all the sciences of observation, human pathology has, in the very nature of its subject-matter, the greatest difficulties to contend with?

I have long and often felt that, in these difficulties, we might gain help from studying the consequences of injury and disease in the structures of plants. For although these, too, are complex, minute and hard to analyse, yet

they are less so than are the structures of any but the very lowest animals ; and, which is most important, the processes in them are not subject to the influence of a nervous system, or of a common nutritive fluid distributed from a central organ and quickly carrying to every part materials derived from all the rest. In the absence of a nervous system and of a quickly circulating nutritive fluid, we come nearer to the opportunity of studying a really elemental pathology, nearer to the simplest changes wrought between formed and varying formless matter. I say, without a nervous system, and without a circulating fluid ; for though some of the properties of vegetable structures, such as are shown in their subjection to the influence of anæsthetics, their movements in relation to light and the various groupings of their colouring particles, indicate a likeness to the properties of simple nervous structures ; and though, through communicating minutest channels, every portion of a plant may be regarded as in continuous relation with all the rest ; yet these small degrees of likeness can scarcely detract from the great contrast between plants and animals in the having, and the not having, nervous systems and circulating bloods.

It was in the hope of promoting a study of such elemental pathology that I determined, when I had the honour of being asked to preside in this Section, to put together various facts in vegetable pathology which, in the idleness of vacations, I had learned. I thought that I might provoke some of my hearers to a study which I supposed that few had pursued. I was nearly turned from my purpose when I found the vast quantity of good scientific work that has been done in it ; the many volumes of books and the hundreds of papers relating to the injuries and diseases of plants which have been written within the last twenty years by both botanists and cultivators,

especially by those engaged in the schools of forestry and fruit-culture in Germany. I saw no hope of studying them worthily ; but, in those that I have had time to read, I have seen enough to make me more than ever sure that human pathologists may find, in watching the consequences of injuries and diseases of plants, facts of the highest interest in their more proper study.

Let me point out some few of these ; but, first, I must disclaim all pretence of an ability to treat the subject as a scientific botanist or a practical gardener or anything intermediate between these excellent extremes. I can only, as a human pathologist, see that knowledge of great value is within your reach : I may be able to point out to you where and how some of it may be found ; and I can promise attractions of novelty in the study and of renown to be gained from it ; for, admirable as is the work already published, very little of it has been done by those who could fully estimate its value in human pathology.

In my selection of subjects, I will limit myself to those that are of interest in General Pathology ; and, first, to hypertrophies, or the simple overgrowths of natural, or nearly natural, structures. Of these, I do not know that there are in plants any well-marked instances corresponding with those which are the most frequent in animals, in which a part grows in direct proportion to the exercise of its function and thus may grow to relative excess when its function is, through the will or for necessary adjustment, excessively discharged. But, in plants, there are abundant opportunities of studying those forms of hypertrophy which depend on an increased supply of nutritive material to any part. To mention but one : the arts of partial or complete 'ringing,' and of constricting or bending the branches of trees, depend for success on their insuring an accumulation of nutritive sap

in the part of the branch from which its movement is checked. The result is what we may call an hypertrophy of flowers, or, in other instances, of fruit, or wood, or bark, proportionate to the increased supply of nutriment; just as, in ourselves, hairs and some other structures will grow excessively with an excessive afflux of blood; or, still more and in almost exact parallel, as limbs will grow with a retarded reflux of lymph.

Similarly, I believe that you may find in plants many instances of the kind of hypertrophy which, in ourselves, we call compensatory, in that, one part being repressed or removed, another enlarges with growth of natural structures, as if for compensation; for I think that some of the arts of gardening are based on observations that removal of certain parts of plants will induce an overgrowth of others. Perhaps, among these you may find facts which may help to explain those obscure cases in animals and ourselves in which parts or organs, which have no natural alliance in function, are yet commonly allied in disease, as if they were in some relation of complementary nutrition. Such are the excessive growths of connective tissue and fat which may follow the removal of the generative organs; events well known but, I believe, not well explained.

I will not cite more facts. Only consider how many of the luxuries from our orchards and gardens are really such growths as in our pathology we should call hypertrophies, and that to produce these is within the art of the gardener, and you may see how large a collection of facts which may be useful in our study are within our reach, constantly at hand for observation and, which is yet more important, within the range of experiment.

Let me speak next of some illustrations of the simple

defects of nutrition which we class as atrophies or degenerations.

I am not sure whether there be among plants any atrophies by mere wasting of parts once completely formed, which may be compared with the emaciation of animal structures.¹ Shrivelling, drying, withering, and defective growth are only too frequent; but these are all different from that unmaking and removing of structures which we trace in both the healthy and the morbid processes of nutrition in animals.

The atrophies with degeneration are very numerous;² but I will take for illustration only that group which we may see beginning all around us in the decay and fall of leaves. This decay is their senile degeneration. It is marked chiefly by their changes of colour—the changes to which we owe the characteristic beauties of our autumn scenery. These are accompanied by changes of texture, shown in the dryness and brittleness of the leaves, and by changes of chemical composition; but I will refer to only the changes of colour. And observe that these indicate decay, not death. They do not occur when fresh leaves are quickly killed, as in hot water, or when they are pressed and dried for a herbarium.

¹ I may have overlooked many instances of it; but some that may seem like mere wasting are rather diseases with associated degeneration. Such are the gum-disease (gummosis), the resin-flux (resinosis), and others of the same group, which I find classed as liquefactive diseases, and in which cell-walls, wood, and other structures dissolve in or into morbid products. See Sorauer, *Handb. der Pflanzenkrankheiten*, p. 184; and Frank, *Encykl. der Naturwissenschaften, Pflanzenkrankheiten*, p. 369. I venture to guess that these may give help in the study of our mucoid and other liquefactive degenerations.

² I greatly regret that, before printing the address, I had not seen Dr. Cunningham's essay on the 'Effects of Starvation on Vegetable and Animal Tissues' (Calcutta, 1879), and his descriptions, especially, of the similar fatty degenerations ensuing in both. I have read no better example of the knowledge which a good pathologist may gain in the study of diseases of plants.

Among many things to be observed in the changes of colour, let me first ask you to note their usual symmetry. One of the characteristics of mere degenerations, as we see them in old age, is that they are symmetrical. I hardly need cite instances; many of us may study them in ourselves or one another. Symmetrical we become, equally on right and on left, bald, or grey, or wrinkled, or dusky with dark pigment in our epidermis, and harsh-skinned with thickening and hardening of its cells. And we know that, as a rule, arteries become symmetrically fatty and calcareous; and that very commonly joints are symmetrically affected with the arthritis of old age; and so on. Now, similarly, the rule in leaves is that, in so far as they are symmetrical in shape and structure, so are the changes of colour which mark their decay or degeneration.¹ You may find, indeed, very many exceptions to the rule; for it cannot be observed in leaves which have been unequally expanded, or whose several parts have not been equally exposed to heat and light, or in which parts have been killed or injured. Many accidents may hinder the observance of a rule of symmetry; but the observance cannot be an accident: and if you will pick-up leaves enough, and look well at them, you will see that the general rule of symmetry in the changes of decay is as evident as is the similar rule in our own symmetrical diseases and degenerations.

The changes may best be seen in those leaves or leaf-lets of dicotyledons which have a simple bilateral symmetry, a median vein or fibro-vascular bundle passing through the length of each leaf and giving off side-branches; but it is often scarcely less plain in palmate and in pinnate leaves, both in their several divisions and

¹ 'In so far,' for the symmetry of living things is not mathematical; it is artistic, in the divine perfection of art.

in the comparison of each division with its opposite fellow. In such leaves you may trace, in each pair of similar parts, similar changes of tint spreading uniformly or gradually over them; usually, first, from a darker to a paler green, from brighter to less bright; then to pale yellow or brown, or to some tinge of red or scarlet or flame-colour, or some other of the tints that make autumn scenery glorious.

Now let me point out some things in our pathology which these facts may illustrate. The changes of colour are not mere chemical changes ensuing in a dead part. Leaves do not usually die till after, sometimes long after, they have fallen. Their changes of colour and of texture, even to the last dull brown in which they crackle as we tread on them, are vital changes in the same sense as those which we see in ourselves in the advance of old age. In this view, they may be taken in evidence on the question as to the nature and meaning of some symmetrical diseases.

There are at least two reasonable theories concerning these diseases. One is, that the symmetry is due to the relation between some morbid material in the blood and certain parts which are symmetrically placed and are exactly identical in composition; absolutely like to one another, and not absolutely like to any other part which is not similarly affected by the same material in the blood. This was the theory on which Dr. William Budd and myself wrote our essays on the symmetry of disease nearly forty years ago. The other, which at the time I only ventured to suggest, but which is now, I think, rather dominant, is that symmetry in disease is determined by the disturbed condition of symmetrically distributed nerves, the disturbance issuing from a single nervous centre. I cannot now discuss this question, or endeavour to show how probable it is that each theory is

true and sufficient for certain cases, and that for some they must be combined; but the symmetry of decay in leaves may be taken as a strong fact in support of at least these two principles: 1. That symmetrical changes may occur in degeneration and disease, as certainly as in development, without any influence of a nervous system or of a circulating blood; and 2. That, among structures which, to all our tests, may appear identical, those alone may be absolutely alike which are symmetrically placed. Surely no two things can appear more alike than are two adjoining leaves of a tree, or two parts of the same leaf; and yet, if, under the same conditions, these do not decay in the same time and measure, it can only be because they are not absolutely alike. Thus the symmetry of decay in leaves may prove that none but symmetrically placed parts may be verily alike; and thus the possibility, to say the least, that in ourselves a diseased blood may similarly affect only such like parts; and thus that, among symmetrical diseases, there may be some of which the essential and sole necessary condition is some morbid material in the blood.¹

But let me now point out another of the lessons which may be read in the decaying leaves; for, really, the pathologist may find in them as many as the moralist and the poet have found.

The leaves, I have said, are decaying, not dead; and their fall is due to other degenerative yet truly vital changes. Dead leaves do not similarly fall. If a branch have been killed before autumn, you may often see its

¹ In connection with this subject, it may be useful to study the variegations of leaves, in some of which, as in many varieties of *Begonia*, a perfect symmetry of colours is observed; in others, as in many laurels, complete asymmetry. Instances of completely unilateral decay may sometimes be found in cabbage-leaves and turnip-leaves near the roots; sometimes, also, in laurel-leaves. They may be suggestive in the study of unilateral organic diseases, some of which are not wholly due to disturbances of trophic nerve-force.

dead leaves hanging dry and withered all the winter through; and often, when leaves are yellow and withered in their last decay, they hang quivering and spinning, ready to fall, yet waiting. Each leaf is literally hanging on a thread; and at last, by a rougher wind, or a drop of rain, or some chance-violence, the thread is broken, and the leaf falls.

This breaking of the thread is preceded by degenerative changes in the structures both of the leaf-stalk and of the stem, adjacent to their juncture or articulation. These changes were first well described by the late Dr. Inman¹ of Liverpool; they have since been made more fully known by von Mohl² and others; and the rules observed in them hold, with certain variations, in the fall of petals, bracts, fruits, some twigs, and other deciduous organs.

In their beginning and maturity, the structures of the leaf-stalk and the stem or twig on which it rests are continuous. There may be some external mark of distinction; but within there is exact continuity; the epidermis, parenchyma-cells, fibres and sap-vessels are alike continuous. But, in preparation for the fall, changes ensue in the adjacent parts of both leaf-stalk and stem. In both, alike and equally, the cells multiply by partition; and those most nearly adjacent change, by a process of degeneration, into cork-cells, dry, brown and air-holding. Then, as these degenerative changes advance from opposite directions towards the plane of junction between leaf and stem or twig, they meet, and, at their place of meeting, an intermediate layer, or rather two layers, of cells die and become scale-like and part asunder; and now the leaf is ready to fall. It hangs only on the dried thread

¹ Hentrey's *Botanical Gazette*, 1849, p. 59, from the Proceedings of the Literary and Philosophical Society of Liverpool, No. 4, p. 89.

² *Botanische Zeitung*, 1860, Jahrg. 18.

of fibres and vessels which pass into it from the stem ; and the stem is protected by its layers of cork and withered cells from the invasion of parasites and insects.

It would be hard to find a more admirable instance of processes adjacent, coincident, concurrent to a common end, yet independent. We have many of the kind in our pathology, but none more evident, or more within reach of complete study, as of vital processes tending to one end, but not guided from one centre ; concurrent, but not concatenate ; as independent as are the works of the several bees that make one honeycomb. And thus we may learn from the falling leaves a lesson against thinking that, when we see concurrent morbid processes, we must always expect to find some centre from which all are guided. It is not to be doubted that in organisms such as ours, in which the work is more divided according to its kind and more distributed to appropriate organs, more is subjected to regulation by central organs, and the working of each part is more influenced by that of all the rest ; yet it is not probable that, in any instance, the law is abrogated according to which each elemental structure lives its own life in a method determined by its own inherent properties. There is no principle in pathology more important than this : let the falling leaves remind us of it.

And yet one lesson more. That thread on which for a time the falling leaf hangs quivering—that thin bundle of fibres and vessels connecting it with the stem—is regarded as a development from simpler cell-structures. Fibres and vessels are ‘higher’ structures ; so much higher that the relation between the Vasculares and Cellulares in plants may match in importance with that between the vertebrata and the invertebrata in animals. But mark this instance of anomaly in our language.

In the elevation from the lowly cellular state to the higher dignity of fibres and vessels, there is, indeed, an instance of that development which makes fit for membership in a higher economy; that is, an economy more nearly like our own. But in the attaining of this fitness there is loss of vital power. There is no such activity of organic life in the vessels and fibres of wood or bark as there is in the cells around them; they are comparatively unchanging; they cannot multiply; they cannot repair their own injuries; cannot protect themselves; cannot even degenerate as the fading leaf-substance around them does (for even to degenerate needs vital power); they can only die, and the dead thread on which the decaying leaf hangs cannot dispart itself; it must be broken by some alien force. In the taking of higher form, the cells seem to have spent their power of forming.

Now we have in ourselves similar instances of degenerations which we call developments. When cartilage becomes bone we usually say it is developed; but this is only because it becomes fitter for a share in a higher condition of our economy. The man is 'higher' than the child, and therefore we are ready to speak of everything as ennobled if it contributes to his manliness. But, in respect of texture and self-activity in vital process, in the activity which can work with even a distant supply of blood, cartilage is better than bone; and the change into bone partakes of the nature of a calcareous degeneration; in general utility there is development, in self-activity there is degeneration.

The same may be said of many changes from cells to fibrous structures in ourselves. In teleology there is elevation; in vital histology, degradation. And this anomaly of words is found, not without some confusion of thought, in parts of our pathology. We speak of

rickets as a hindered or arrested development of bones, and so, in respect of purpose and utility, it is; but, in respect of elemental tissue-life it is rather an arrested degeneracy.

You may study such anomalies of terms in many instances in plants. Let me suggest a thesis for the D.Sc. There are such things as green roses: show the analogies between a green rose and a rickety child.¹

But now let me end my notices of those changes in plants which you may study for illustration of atrophies and degenerations and retrogressive metamorphoses in ourselves. There are many more, such as the sweetening of fruits, in which, while becoming more useful to ourselves, they acquire chemical conditions less distant from inorganic matter; and the red and yellow tints of ripeness, which are colours indicative of imperfection; or the central softening and decay of fruits very like that of large morbid growths. In all these, as well as in decaying leaves, you may study general pathology when and where you will.

I will next speak of the repair of injuries.

In studying the processes of repair in plants a first observation may be that, speaking generally, the repair of injuries and diseases is less complete in them than it is in animals, even than in ourselves and others of the highest groups. The processes of repair, so far as they extend, are similar in both, but in plants they rarely extend further than to be defensive or protective to the injured parts; they seldom reach to what may justly be called reparative or reformative, and much more rarely to anything that may be named reproduction or regeneration of lost parts.

¹ Help may be found in the study of Dr. Ord's thoughtful paper 'On Brownian Movements' in the *Journ. Micros. Soc.*, June, 1879.

Indeed the only instances of reproduction which I have found recorded, are those of the cells of *Vaucheria*,¹ and of parts of the minute leaflets of the small moss, *Bryum Billardierii*, and even in these the mid-rib was not reproduced.²

In most of the ordinary wounds and other injuries of leaves and stems, as well as of petals and fruits, we see only incomplete repair. Punctures and incisions and minute excisions such as in ourselves would be quickly closed and healed, and in a few days be almost unobserved, are, in plants, not closed at all, unless with careful help and in some few species.³ The gaps or spaces are not filled-in with new material, there is no tendency to contraction, the edges do not draw together. The healing which does take place in these cases may be compared most nearly with that which, in animals, we see in wounds left open and healing under a scab. The cells at the ends of the fibro-vascular bundles which are cut across empty themselves and die, and then, becoming dry, they slowly scale off and are detached: or, the contents of the cells may ooze out and yield a gummy deposit which may dry and glaze the surface of the wound. After a time this cracks and is detached, but while it lasts it serves, as do the dead and dry layers of divided cells, for some protection of the cut surfaces. They serve, alike, for timely protection against invasion by parasites, just as in an animal do the dried blood and the first exudation on a wound left open. Beneath this temporary protective layer the more permanently protective or

¹ As quoted from Hanstein by Frank in the *Encykl. der Naturwissenschaften*, Abth. i. Lief. 12, p. 380; and in *Botan. Zeitung*, 1873.

² Described by K. Müller in *Botan. Zeitung*, 1856, p. 200, and quoted by Frank, Waldenburg, Beijerinck and many more, who do not add any similar fact.

³ See Frank, *l. c.*, p. 383-4.

reparative changes now ensue, and these are usually described as in two methods; the formation of cork and the formation of callus.

In the healing with cork, which is the more frequent in the more succulent parts of plants, the layers of cells next beneath the dead cells or the gummy covering multiply by frequent partition, forming rows of thin-walled flattened cells. Then of these the outermost, lying at and next to the surface of the wound, become cork-cells, their walls thickening and their contents being replaced by air. The more deeply seated remain active, still capable of multiplying and of replacing the cork-layer, which now serves as a dermal covering, and becomes continuous with the periderm at the borders of the wound, but does not acquire an epiderm.

In the healing with callus, examples of which may be studied in the closure of the wounded surfaces of slips and cuttings, the living cells next to the surface of the wound grow out into papillæ or short pouches, with or without cell-division. These may form a protective cork-layer, and if they approach from nearly adjacent surfaces they may intermingle and unite; or they may form other structures, replacing those injured, as epithelial cells, parenchyma-cells, or a new cambium layer, in which bark and wood and fibro-vascular bundles may form.

The best instances of healing after this manner are in the wounds or the splittings or strippings of bark which do not go through the outer layers of the cambium in vigorously growing stems or branches.¹ In the actively living structures of the cambium, if it be very gently

¹ It may be well studied in longitudinal wounds made through bark for the relief of tension (See H. de Vries in *Archives Néerlandaises*, t. xi.; Haarlem, 1876).

guarded, callus forms by outgrowth and division of cells, somewhat after the manner of granulations, and from these not only bark but wood, vessels, and cork may form and become connected with the similar tissues at the borders of the wound.

These, I say, are examples of the best healing known in plant-structures, and they must be worth studying carefully, for they would illustrate many points in our own healings and are far more fit for minute examination than any injured structures in animals.¹ Yet even in these the defective reparative power in plants is shown. An animal, if skin be stripped from it, will, if possible, lick the raw part, and then let dust and hair and dirt collect and help to make a scab with blood and oozing fluid, and under the scab new skin will form, not indeed perfectly but well. But when bark is stripped only down to the living cambium the raw part must not be touched; mere wiping or touching with the finger, or too much drying, may kill the cambium-cells and prevent all healing; or parasites may attack it and make more mischief. And when we leave these best cases the repairs observed in plants are far less; even small branches or twigs when partially broken do not reunite, unless in a few instances in succulent plants in which the broken surfaces are carefully kept in contact, and even in them no new woody tissue may be formed. You know what is called in surgery a greenstick-fracture and its sure method of repair. In a real green stick no repair of the kind would happen; no ensheathing or intermediate callus would form; at the best the broken surfaces would be covered with a protective layer of cork without an epiderm. I cannot

¹ I have adopted, chiefly, the descriptions of healing given by Frank (*l. c.*). I hope that more minute descriptions and more mutual illustrations between animal and vegetable repairs will soon be published by Mr. Shattock, who has been studying the subject with great care and skill.

find an instance of repair in plants which even approaches in completeness to the ordinary repair of a broken bone or a divided nerve or tendon in ourselves or any other animal.

Perhaps the strongest contrasts may be seen in the wounds or strippings of trees that go down to the layers of wood; or, in what will follow the amputation of a branch from a tree and of a limb from a man. You may see in front of St. Catharine's College some elms from which branches were cut yesterday; and not far off, at Addenbrooke's Hospital, I dare say you may see patients from whom parts of limbs have been amputated, and if the stumps have been treated after the manner advised by our distinguished President, they will have been left to be repaired by nature, as truly as are the stumps of the branches of the elms. The stumps in the hospital have been cut in better shape for healings; but those in the garden have the advantage of more perfect rest, and in whatever shape they might have been cut, the result would have been the same. Now, it is most probable, nearly sure, that the human stumps will be healed in a few weeks, and that in two or three months all the wounded parts will be covered-in with skin and scar and cuticle firmly adhering and compact. But it will need a very long life to see the healing of any stump of the same size on the trees. The wood cut across will die, and its necrosis will never be repaired; rotting, like a caries, may extend from it into the trunk, but it will not be repaired. Year after year the annually formed rings of new wood and bark will overlap the borders of the wound; each year they will grow thicker and will converge, and at last after many years they may, perhaps, meet and coalesce and cover-in the wounded surface; but they will not unite with it, there will always be a cavity over the dead wood:

a stump so ill-healed as would be a shame to any hospital. I must not imply that there are no better healings after amputations in trees than you may see in elms: some trees heal better, as do beeches and planes; but I think it may be said generally that the stump of any branch will need more years for its healing than the stump of a limb of the same size will need of weeks; and that, in the end, the work of the weeks will be far more reparative than that of the years. And in this reckoning I would include not only the limbs that have been surgically cared for, but those that have been violently crushed, mutilated, or torn off in animals left to themselves.

This slowness and defect of repair are observed in other things besides the healing of wounds. Dead portions of leaves, or of wood, or other structures, are not cast-off unless by external force.¹ We have borrowed the word 'exfoliation' to tell of the separation of dead bone; but, as I have said, it is only living leaves that can exfoliate. Dead pieces of young wood remain where they were killed, as they often are, without external wounds, by hailstones² or any accidental blows. The structures adjoining them may become hard or corky, but they do not separate from them; there are none of the reparative changes which are seen in even a 'quiet necrosis.' Similarly you may often see dead tips of leaves, perfectly defined from the living parts by colour and by texture, yet without any groove or other sign of process of separation, the dead and the living epidermis remaining still continuous. And as to recovery from diseases I believe it may be summed up in saying that no vegetable

¹ A partial exception must be made for the tips of the fibres and vessels divided in some cuttings. See Arloing, *Sur le Bouturage des Cactées*, *Ann. des Sc. Naturelles ; Botanique*, B. iv. p. 24, 1876.

² Goethe, *Ueber den Krebs der Apfelbäume*, 1877, p. 2.

structure altered by disease ever reverts to its original healthy state.¹

Now let me commend to you this subject of the repair of injuries in plants, not only for illustrations of our pathology, but as a part of the study of what may be called the philosophy of healing. They heal very slowly and imperfectly, they may need many years for the healing of small injuries, eight or nine years for a frost-crack in a young branch : ² yet they do heal ; the intention, as Hunter would have called it, is not given up. Let me relate one case.³ A fir tree, fifty years old, had a large piece of bark stripped from its trunk. The wound extended round nearly a fourth of the circumference of the trunk, and laid bare the wood. It was not dressed or guarded : the outer layer of the exposed wood died as usual, and then every year the successive annular growths of new wood and bark extended a little further over the bared place. In one hundred and fifty years these growths met and coalesced, and the wound was covered-in. When the tree was felled and cut through at the injured part, where there was still a deeply depressed scar, the concentric rings of wood proved the growth of fifty years before, and one hundred and fifty after, the injury, and even now the healing was not complete ; there was still a cavity between the old wood and the new ; the healing was not such as we should call good in any similar case in an animal. Still, there was healing ; the 'intention' was maintained for a century and a half.

¹ 'It is confessed that diseases in the vegetable kingdom, when once established, are, for the most part, uncontrollable.' Berkeley, *Gardeners' Chronicle*, January 21, 1854, p. 37. It is greatly to be regretted that these admirable essays on Vegetable Pathology were not collected and published in a single volume.

² Goethe, *l. c.*, figs. 3 and 4.

³ From Ratzeburg, *Die Waldverderbniss*, vol. i. p. 98, pl. 21-22, and 31a-32.

Surely, in our familiarity with the processes of repair we overlook their wonder; the wonder that there should be in anything an inherent power of repairing the consequences of accident; that, for instance, in any one of us, after living for many years, with only those timely regular changes, the methods of which we have inherited from our ancestors, an accident, some quite unlikely event, such as may never have happened to an ancestor, such as no conceivable process of selection or of inheritance can have prepared us for, some mere chance as we must call it, should bring out an increased power and a new method of organising structures exactly adapted to the repair of the consequences of that chance.

Let me remind you that this power of repair is not a property of living things alone—it is evident in crystals.

It may seem unreasonable to say that we may study principles of elemental pathology in the apathetic inorganic world, or in things that we call dead. But certainly we may study them in crystals. The experiments of Jordan which I repeated many years ago, and related in lectures at the College of Surgeons,¹ have been extended by Pasteur,² Karl von Hauer,³ and others, and this chief fact has been abundantly proved—that if a portion of a crystal be broken off, or filed or dissolved away, and if then the mutilated crystal be replaced in a solution of the same salt or of an isomorphous one, the lost part will be replaced, the damaged form will be repaired. In this process of repair the whole crystal will be enlarged, new crystalline matter will be formed on every surface, but the quantity formed on the injured part will be greater than that formed on any other part, and repair will be more active

¹ *Lectures on Surgical Pathology*, p. 118, ed. 3.

² *Ann. de Chimie et de Physique*, t. xlix. 1857, p. 6.

³ *Sitzungsberichte der K. Akad. der Wissenschaften*, B. xxxix. ; Wien, 1860, p. 611.

than mere growth, till the proper form of the crystal is regained. Then, when the repair is complete, growth alone will go on, and each part of the crystal, if it remain in the same solution, will increase in due proportion with the rest.

It seems impossible, by any just definition, to separate this process from those which, as pathologists, we study in the repair of the accidental injuries of living bodies. In all alike, when a natural form has been lost or impaired by external force, the native formative power works at the injured part with an energy and a method modified appropriately to the recovery from the loss, and then, when the recovery is achieved, returns to its previous energy and method. Of all alike, it may be said that where we find evidence of a design to be fulfilled in the attainment or maintenance of a certain form, there, also, we may find evidence of some power to repair injuries which that form may sustain from accidental external forces.¹ However various in its degree and method, the power of repair is in all.

I wish that some of you, with a good broad knowledge of pathology, would study this repair of crystals and the possibility of their recovery from other changes besides those of form. The study could hardly fail to yield facts that would be very useful in the proper business of our lives. Only let me warn you that, as I have learned from Mr. Maskelyne, injured crystals need great care ; if they are to be well repaired they must have as gentle handling and as kindly nursing as the tenderest patients of our own race.²

¹ In the repair of crystals, it is well seen that form, rather than size or other quality, is the highest object of the reparative process. Karl von Hauer has found that if a portion of a crystal be so removed that all the parts of the remaining mass are still within the law of its primary form, the repair is either very slow or is even not attempted.

² Yet their healing may be 'left to nature.' Mr. Maskelyne has often seen in native crystals marks of injuries repaired.

Even if, in studying the repair of injured crystals, we should not learn things directly useful in our own pathology, yet the facts should keep us safe from the error of referring reparative processes to such supposed causes as are called increased action, or determination, or reflex trophic force, or the like; they must convince us that repair is the result of some elemental general law of formed matter prevailing beyond the whole range of life and living structure, even in all natural bodies having harmonious form and proportions.

But while, in the widest survey of repair that we can take, we may thus see an instance of so simple an expression of the Divine Will as we call a general law, yet, in the degrees of apparent observance of that law, we find differences very wide and, so far as I know, quite inexplicable. We do not find equality of reparative power in all formed bodies, or any rule of proportion between that power and their less or more simplicity of structure. There are some rules of such proportion among animals, but they are very partial, and the rule fails completely when we compare the reparative processes in plants with those in animals.

Let me revert to this comparative defect of healing power in plants, though it may be only to tell my ignorance of all relations between it and any other of their properties or the conditions of their life. It is not related to any immunity from injuries enjoyed by them, for they are far more exposed to injuries than are animals; nor to any absence of necessity for healing, for, after wounds, they suffer severely, and often fatally, from the attacks of parasites of all kinds. It is not because of any lack of productive power at the injured part, such as we might be disposed to refer to the want of circulating blood or of nerve-force; for round many wounds of wood hard and

heaped-up masses of new wood may form, such as may be roughly compared with our cheloid scars and excesses of bone-callus. From many wounds of plants, as the arts of grafting and of multiplying by slips or cuttings may show, entire new plants may grow ; and yet these are not reproductions ; they may, in one sense, replace that from which the slip was cut away, but they do not reproduce it ; they are not continuous with its remains, and they commonly become much more than that which was lost would have become. None of these increased growths which injuries may produce in plants are to their own advantage, though they may be so to that of a successor or a parasite. Neither can the defective repair be ascribed to any general tenacity in the regular methods of growth in plants, or to any general want of pliancy or of ability to adjust themselves to new conditions ; for the art of horticulture abounds in instances of changes in methods of growth completely adjusted to changes of condition. Thus there are in plants ample powers for enlarged growth and ample range of pliancy in method ; but that special change in method of growth, which is familiar to us in ourselves and other animals, and which, as with design and on purpose, is adapted to the speedy repair of injuries, is in them comparatively defective. Who of you will work so as to find the explanation of that which seems so strange an inconsistency ?

The resemblances which, in human pathology, we trace between the process of repair and some of those of Inflammation are well-marked in plants. Of course, in speaking of inflammation in plants, we must be nearly limited to the essential parts of the process. In ourselves, we look to its so-called signs—heat, redness, pain, swelling, and disturbed function : but these are not of the essence of inflammation and, even in ourselves, one or

more of them may be absent when yet the real process of inflammation is active. For the elemental pathology of inflammation, as for that of decay and of repair of injuries, we must study the altered relations between the elemental formed parts and the varying formless materials about them ; and these, let me repeat, may be seen more nearly in plants than in animals, in which it is hardly possible to isolate any part from the influence of stagnant or wandering corpuscles or of circulating blood and nervous systems.

The signs of inflammation, I have implied, cannot well be observed in plants. Increased heat in the part has not, I think, been noticed ; yet I suspect that, with so rapid organisation as sometimes ensues in gall-formations, it might be detected. Redness is often present, as in the galls and curled leaves of which I shall speak presently ; but it is in a fallacious likeness to the redness of congestion of blood ; it is only the colour of imperfection which one sees in many immature and many decaying leaves. Pain we cannot suppose in plants ; and yet it may be through some change like to that of transmission of nerve-force that we must explain what I find told by Frank ;¹ that in many green parts of plants there appears, some few minutes after wounding them, a change in the normal position of the chlorophyll-grains and the protoplasm in cells at a distance from the wound—a change which, as he says, must be regarded as due to the transmission of a stimulus of some not well known kind from the wounded to the intact living cells. Slight slowly ensuing swelling is often evident in the inflamed parts of plants, and is due, as in animals, to new formation and to increased afflux of nutritive matter—an afflux,

¹ *Encyklopädie der Naturwissenschaften*, Abth. 1, Lief. 12, p. 338, from a paper by him in *Pringsheim's Jahrb. für wiss. Bot.* viii. p. 220.

be it observed, not due to any vasomotor nerve-change. Disturbed function is evident enough.

But the likenesses between the inflammations in plants and in animals are best shown in their visible structural changes; and these have been admirably traced by Waldenburg.¹ He has applied various irritants to leaves, fruits, and stems—such as foreign bodies, setons, crushings, cauteries, and others. The results, speaking very generally, are that, as in ordinary wounds, the cell-structures actually involved in the injury perish and dry up; that those most nearly adjacent suffer degeneration, indicated by their protoplasmic contents becoming turbid and their chlorophyll becoming yellow or brownish; while in those next to them, and within distances varying according to the injury and the texture of the part injured, enlargement of cells ensues, and increase by division and thickening of the cell-walls.

In these changes you may study, with comparatively easy experiments, imitations (as near as difference of texture will allow) of the most constant constituents of inflammation in animals, especially those of the least acute of the productive interstitial inflammations leading to thickening, opacity, induration, and other such changes. The defective maintenance of some of the natural structures of the part is evident; so is an increased afflux of nutritive material; so is an increased production of lowly organised structures, not widely different from the natural structures.

But, of all morbid processes in plants, none, I think, are so suggestive as are those produced by parasites, whether vegetable or animal. The whole subject would be far too large to speak of, even if I were familiar with it;

¹ Virchow's *Archiv*, Band 27, 1863, p. 145.

it is, indeed, a subject of the gravest national importance ; but, keeping to the design with which I started, and which was only that of pointing out where useful pathological knowledge may be gained, I will speak of only some of the changes which are produced by insects. The most remarkable of these are the galls ;¹ and, among the many hundreds of them that have been described, I may assume that you know some in their natural mode of growth—such as the common oak-apple, with which some celebrate the restoration of our monarchy ; and the bedeguar of the wild rose ; the bright crimson oak-spangle, the currant-gall, or the artichoke-gall, or the gall of pharmacy. But, besides the hundreds of different true galls, there are still more hundreds of changes of structure in leaves and stems and roots, all produced by the irritant secretions of insects, and all such as may justly be ascribed to processes of inflammation. In some, as in the ‘curl’ of the leaves of the whitethorn, you find thickenings of leaves which are lifted, rolled, or curled into chambers, which serve for defence of the aphides or other insects ; in some, the thickened and distorted clusters of leaves, in buds or on twigs, roll up and are

¹ Many growths like galls are produced by fungi and other parasitic plants ; but those I omit. Many are described by Schroeter, *Beiträge zur Biologie der Pflanzen*, Band i. p. 4 ; and by Beijerinck. The best essays on the minute structures and modes of growth of galls are those of Lacaze-Duthiers in the *Ann. des Sciences Naturelles, Botanique*, 1853 ; of Prillieux, in the same, *Botanique*, 1876 ; and of Beijerinck, *Bijdrage tot de Morphologie der Plantegallen* ; Utrecht, 1877. But I am most indebted to Miss E. A. Ormerod, who, by her admirable works on Entomology and Meteorology, sustains the scientific reputation of the name so well known to pathologists. Since the Address was sent to press, I have received the second part of Frank's essay in the *Encyklopädie der Naturwissenschaften*, containing a much fuller account of galls and gall-like growths than, I think, had before been published. With this, or with the soon-coming second part of his separate *Pflanzenpathologie*, and with Beijerinck's essay, the whole subject may well be studied by any human pathologist. But I hope that it will soon be yet more clearly illustrated by Dr. Ransom.

mutually fastened, so as to form the walls of similar defensive lodgings; in some cases, leaves become swollen as with a kind of œdema; in some, their layers separate as if with blistering; or leaves, or stems, or fruits, or clusters of flowers, buds, or roots, produce variously shaped and variously constructed growths of cellular parenchymatous tissue and cork and, more sparingly, of woody tissue or of cells whose thick walls become as hard as wood. Besides, some of the monstrous growths of parts of plants, and some of the viviparous variations, and of the undue metamorphoses of leaves, are to be referred to the influence of parasites.¹

It may seem bold to speak of so many hundreds of widely various morbid processes as having any essential character in common, or as fit to be included under one name; yet, I think we may regard the whole of these as being such as, in our pathology, we should call inflammatory hypertrophies or hyperplasiæ. They all show a rapid increase of lowly organised structures, by derivation from, and in continuity with, those pre-existing. There is, as in the products of our inflammations, a general likeness among these new structures, whatever be the part of the plant from which they are derived, and all bear a general likeness to the structures formed after injuries of actively growing parts. In the morbid growths formed by these new structures, the deflection from the natural shape and construction of the part, in continuity with which they have grown, is often not complete; they often retain marks of characteristic normal forms, and sometimes

¹ Among them are the 'witch-brooms' of the birch, pine, larch, etc. Miss Ormerod has described the 'Phytoptus of the Birch-knots' in the *Entomologist*, 1877. A long list of the teratological forms due to parasites is in Beijerinck, *l. c.*, page 32; with this should be studied Dr. M. Masters's volume on Vegetable Teratology, and much help may be gained from the article on Galls, in the new edition of the *Encyclopædia Britannica*.

acquire marks of natural variation from the species.¹ Moreover, all these morbid growths have their origin in what may justly be called 'irritation' of the part on which they grow; and in all of them, I think, we may note signs of degeneracy from natural conditions, either in the absence of stomata or similar structures, or in the presence of the red, or yellow, or other colours commonly noticed in decay.

Here, I believe, are reasons enough for regarding all these galls and gall-like products of disease, generated in plants by insects, as analogous with a large group of the products of inflammation which we study in our own pathology; and the analogy is not the less because neither group can be circumscribed with any exact definition.

I will not be tempted to speak long, but I beg you to think long, of the marvellous facts of natural adjustment which we have here, in this intense example of the '*sic vos, non vobis*.' Here are the bare facts. Each species of these parasitic insects can compel some part of a plant into such disease as shall supply good food, or well-built and well-placed lodging, or both, for itself or for its eggs and larvæ, or even for part of the life of its complete offspring. Each insect selects, by instinct, the very part of the plant which is adapted to its purpose. The provision made at the cost of the plant is exactly adapted in quality to the welfare of the insect or its offspring, and in quantity as well; for both the lodging and the food are made sufficient for any necessary time—for days or weeks, often for many months; in some instances, for two or even three years. Nay, more than this, a gall, of which the

¹ As Darwin pointed-out in the likeness of the mossy covering of the common wildrose-gall to the calyx of the moss-rose. There are, also, instances in which galls imitate small cones or other structures of the plant.

growth has been provoked by the virus of one insect, may be fit for the food and lodging of another, which, when all seems complete, can penetrate the gall-cavity, and there, as with theft or murder, obtain food and lodging perfectly suited to itself or its progeny.¹ And the whole process in the plant, though it be one of disease and, in a sense, unnatural, is yet so regular, so constant and specific, that the form and other characters of each gall or other morbid product are, usually, as constant and characteristic as are those of the insect itself, and the differences among the galls are at least as great as those among the insects. Is there, in all the range of natural history, a more marvellous group of facts than may here be studied? If you would like to work-out a problem in evolution, find how it has come to be a part of the ordinary economy of nature that a gall-insect compels some part of a plant to grow in a manner which, while injurious to the plant, becomes useful to one insect not yet born, and to another who will, in due time, invade the gall and kill and feed upon its occupant, and then may itself be invaded and eaten by a third.

But now, of the relation between galls and our specific diseases, such as our eruptive fevers, syphilis, cancer, gout, and others.

In these galls and other similar diseases in plants, we have, it seems, hundreds of specific diseases due to as many hundreds of specific morbid poisons;² for the most reasonable, if not the only reasonable, theory of these diseases is, that each insect infects or inoculates the leaf

¹ The Inquilinæ, Einmieter, lodgers, or (as they might be called) burglars, are usually described in the works on gall-insects, though not themselves 'gall-makers.'

² Karsch (abstract in the *Botanische Jahresbericht*, 1879, p. 150) gives the total number of the forms of galls at present known as about 1,250, of which one-fifth belong to the Cynipidæ.

or other structure of the chosen plant with a poison peculiar to itself. The poison may be merely deposited; but, in the instances best for study, it is inserted in the plant-structure, whether leaf or any other; and the wound for inserting it, the poisoned wound, may be made either with part of the oral apparatus, or, as in most of the true galls, with the ovipositor through which one or more eggs are passed with the virus, and are left among actively living structures of the plant. The little wound closes; the virus, whether an oral or an ovarian secretion, remains; and the result of its influence on the plant-structures and their contained protoplasm is the formation of the gall or other morbid product. The whole process may be compared with the local consequences of the insertion of vaccine lymph, or any such morbid poison, in ourselves or other animals. I say the local consequences, for we have no clear evidence of what might be called general infection or constitutional disease in the gall-forming plants.¹ In the absence of quickly moving fluid, such as lymph or blood, the virus infects only the part in and very near to which it is inserted. A single oak-leaf may have fifty 'spangle' galls on its under-surface, but the structures between them may be quite healthy; and when in any instance a general damage is done to a plant by gall-growths, it seems to be only as a remote consequence of the spoiling of considerable portions of its structures. And this appears to be true, even though the virus may continue active for a long time, as in the galls which begin to grow soon after the insertion of the virus with the ovum, and continue to increase during the whole—sometimes

¹ Dr. Masters has reminded me that something like infection must occur when a variegated scion grafted on a green-leaved stock affects its leaves, and that permanently.

long—development of the larvæ; or even, in a few instances, after the larvæ have deserted them.

But, while plants supply no occasions for studying fever, or any other of the constitutional phenomena of specific diseases, they are all the more fit for illustrations of some of the local characteristics. Let me suggest some things for study in them, in addition to the inflammatory changes of structure.

We find hundreds of different forms of galls, and we may be nearly sure that there are as many kinds of morbid poisons produced by the gall-insects, each form answering to a different virus. This may suggest that we may be too grudging in thinking of the number of morbid poisons, or of their modifications in the blood, to which diseases in ourselves may, at least in part, be due.

It is true that the galls are produced by many species of insects on many species of plants; and that the differences among these species may be as wide as those between ourselves and any other mammalia. But, even among closely allied species, there are many and very different forms of galls. Mayr,¹ ten years ago, described and figured ninety-six kinds of galls found on the oaks of middle Europe, all but two of them being produced by different species of gall-wasps. Of those ninety-six kinds, thirty-two are formed on the leaves alone; and, even on similar parts of one oak-leaf, it is not rare to find three or four different forms of galls.²

We have, thus, clear evidence of a very large number of morbid poisons, each of which is capable of producing, in an appropriate subject, a distinct specific disease with a characteristic morbid structure.

¹ *Die mitteleuropäischen Eichengallen*, 1870.

² While writing, I have before me some oak-leaves from a sapling on which are five kinds of galls, three being, in several instances, on one leaf.

We may safely believe that, for each of these morbid poisons, there is no test yet possible except that of the disease which it may produce ; and so we may as safely believe that there may be many morbid poisons or morbid conditions of blood in ourselves which may be indicated by very different products of disease, though they may be beyond detection by any other, even the most refined, method of research.

Further, though it may be impossible by any other means to detect any differences among these morbid poisons, yet the diseases which they severally produce may be widely different ; as unlike as are a pustule and a goitre, or a vaccine vesicle and a carbuncle, or as any of the morbid changes due to gout or rheumatism.

It may be observed, however, that these great differences are marked in outer shape and construction much more than in minute structure. As, in human pathology, there are certain general characters and degrees of likeness in all inflammatory products, however differently they may be constructed—in pustules, vesicles, thickenings, opacities, adhesions, scars, fibroid and other changes ; so, in galls, there are certain likenesses in minute structures, even among those that are, in their construction, size, and outer shape, most unlike.

It may be well to learn from this a lesson on the imperfection of our methods of minute research. As we cannot doubt that the differences in outer shape and method of construction of the products of specific diseases are associated with differences of chemical composition and ultimately minute structure, so it must be in those yet greater differences on which we frame our distinctions of species in all living nature. The coarse, visible, and tangible distinctions may be well marked ; the really material differences with which these

are associated, and to which, probably, they are due, are beyond our reach.

Again, in the study of specific diseases in ourselves, we see many variations due to the differences in the parts, or even in the persons, affected with them. In the study of galls, similar variations may be seen. As a general rule, each gall-insect lays its eggs in one part of one plant—as the leaf, leaf-stalk, bud, fruit, or root of this or that species; but if, as rarely happens, one lays in different parts of the same plant, there is usually a very close agreement in the characters of the resultant galls. A few exceptions to this rule are known, one of them being in the very different galls produced on the roots and on the leaves of vines by *phylloxera vastatrix*; but the rule is generally observed, and accords with the fact of certain features of general likeness being observed in the products of our several specific diseases, wherever they may be seated.

When the same insect lays in similar parts of different plants, the galls may be all similar;¹ but I believe that they more usually are different, and that their differences are such as bring them severally nearer to the distinctive characters of the plants on which they grow; just as, in ourselves, a specific disease may be modified by the personal conditions of each patient.

In similar analogy, the differences are yet greater when the eggs are laid in different parts of different plants.

Very rarely, the same insect may produce on one leaf different forms of galls.²

¹ Fr. Löw. Ueber Gallmilcken; in Verh. der K. K. Zool.-botan. Gesellschaft in Wien, 1877.

² Beijerinck, l. c., p. 21, from whom, also, the two preceding sentences are quoted. He cites from Winnertz the case of *Cecidomyia tremula*, which may produce on the same leaf of the white poplar four different forms of

In all these points, you may, I think, find help in the study of specific diseases. I will add only one more. Usually, the gall begins to grow directly after the deposit of the egg; but sometimes there is a long delay, a long period of suspense, an 'Eiruhe,'¹ which may last for many months before the growth begins. What is going on during this time? I believe we may see here an instance of events very difficult to study in our own pathology, in which two or more conditions must concur to the production of some disease, and one of them must wait for the complete efficiency of the rest. In the case of these long-delayed galls, either the egg, after being laid, requires a long time for the completion of changes ending in the production of the necessary morbid poison, or the plant-structure in which it is laid requires the time for changes to make it susceptible of the poison; or both egg and plant may need to change. So, in us, two or more conditions must concur. A tendency to gout may be inherited, and the blood may have slowly acquired the necessary morbid condition; but no structure may be susceptible of gouty disease till a blow, or a strain, or some disturbance of nervous force makes it so. So with cancer; a general tendency may be inherited, but it must wait till the material of some structure is, by age, or injury, or long continued 'irritation,' changed into fitness

galls: but I suspect that there may be as many differences among the eggs; just as there are among those of some *Lepidoptera*, in which, out of the same brood of eggs, all laid in the same place, some are hatched in the next following season, others in one, others in two, seasons later. Of course, these eggs are not all alike, though no other difference besides that proved by the times of hatching may be discoverable in them, or in the insects produced from them.

¹ Schenck, *Beiträge zur Kenntniss der Nassauischen Cynipiden*, p. 24. He cites the case of *Trigonaspis megaptera*, which lays its eggs in May, and the galls do not appear till the next April; others lay in buds, and the galls appear at the time of opening of the leaves. Hartig mentions similar facts (*Zeitschr. für die Entomologie*, Band ii. p. 176, 1840).

for concurrence in morbid action with the material on which the general tendency depends. Then, when the two materials meet in mutual fitness, the result may be a change so great that we may compare it with that from an act of impregnation. I have often thought of this comparison, when seeing the almost sudden appearance of cancer in a breast or a tongue or a scar long irritated. In the growth of these galls, the comparison may seem less far-fetched. At least, it may be difficult to suggest any nearer comparison for a process in which the meeting of two living materials from different organisms is immediately followed by such a change in the method of life of one of them, as ends in the production of a definite new growth exactly adapted to the method and purpose of the life of the other.

But it is more than time that I should have done with galls. If I have been tedious, let me assure you that I am myself ashamed to have gathered so little from the rapidly increasing records concerning them to which the botanists, and still more the entomologists, of our time are contributing. And, even for that little, I feel as if I deserved to be compared with one of those burglars of whom I spoke as feeding on the results of others' labours. Let it be my apology, that I believe I have taken nothing that those others would have used. I have only taken, from their rich stores of facts, some that may be much more useful in pathology than in natural history. And I am sure, from all I know of naturalists, that they will gladly let any of you into their fields, though you may use them for your own purposes much more than for theirs. It would be difficult to find a field for the study of the very principles of pathology larger or richer than this offers to you. The objects are within easy reach; hundreds of morbid processes are at hand for deliberate

study ; experiments may be made at will ; during many months, thousands of insects, as natural vivisectors, will be at work for you ; and it must yet be some years before they will be required to take out licences.

I will speak of only one more of the subjects of general pathology—that of tumours ; and on this I may be very brief.

I do not know if there be any morbid growths in plants which may justly be compared with our cysts or cystic tumours. Foreign bodies lodged in them usually give rise to such changes in the immediately surrounding textures as produce layers of cork-tissue, which may be compared with the capsules of connective tissue similarly formed around foreign bodies in animal structures ;¹ but I do not know if there be any such diseases as cysts filled and enlarging by the increase of secretions or growths from their own walls.

Growths which may be coarsely—and only coarsely—compared with our cheloids are common on elms and other trees, especially on apple-trees. They are called cankers, or in Germany, by the same name as cancers—*Krebs*.² They are usually rounded and coarsely nodular masses of wood covered with bark, sessile singly or in clusters on branches, and depressed at the middle of their projecting surfaces. Thus, in form, they are not far unlike masses of scirrhus cancer projecting from the breast or axillary glands ; and the likeness is the nearer when, as is usual, the depression on their surface leads into a cavity bounded by decaying wood. The imitation of an ulcerating cancer may justify the use of the same name ; but, in the canker of the trees, the central cavity

¹ See Arloing in *Ann. des Sc. Nat.*, 1874, tome iv. p. 27.

² Sorauer, *Handb. der Pflanzenkrankheiten*, 1874, p. 199, etc. ; Goethe, *Ueber den Krebs der Apfelbäume* ; Leipzig, 1877.

usually indicates the beginning, not the advance, of the disease; for cankers, like most cheloids, are formed after wounds—whether from frost, or blow, or injury of parasite, or any other cause. Around these wounds, layers and bosses of wood are annually heaped-up, as if with an exaggeration of the process of repair, and with concentric growth they may close in and complete the likeness to the cheloid. But, more commonly, while the outer part of the canker is annually increasing, its inner part is constantly decaying, under the attacks of the parasites which the wound let-in, and which plants seem to have no power to expel. Thus, within a kind of shell of laminated wood, outstanding like a tumour on the branch, you may find a great hollow space filled with rotten wood and other residues. It may look very like a mass of cancer with central softening and decay; and the likeness is even greater when, as not rarely happens, the central decay keeps pace with the formation of new wood. Then is formed what some call an open cancer, others gangrene or Brand. In these, an uneven surface of rotting and dead wood is surrounded by an undulating, knotted and everted or over-hanging border of the wood annually formed, but in its turn destined to decay, and thus to imitate the constantly enlarging cancerous ulcer with its nodular uplifted borders.

It is impossible not to be struck by these likenesses between cankers and cancers; and I commend them to your study, though I fear they are much more apparent than real. The nearer, yet still distant, likeness seems to be that to cheloid growths; for these also appear as morbid troubled processes for repair, and they are so far like to tumours that no line of definition between them seems yet clear.

The growths in plants which may, I think, be deemed

most nearly like to our tumours, are those which are called exostoses,¹ knaurs, or wens. They may, indeed, be regarded as only prominent examples of that disorderly growth of adventitious buds which produces the various strange and beautiful knots and veins in ornamental woods; but they are too like tumours for me to pass them by. You may find plenty of them on the trunks of the beech, hornbeam, ash, birch, holly, and cedar. The best specimens appear as ovate or nearly spherical masses of hard wood partly covered with bark which is reflected on them from the contiguous bark of the trunk on which they rest. They range from a few lines to many inches in diameter, and are attached sometimes broadly, sometimes by short narrow pedicles continuous with the wood of the tree. Some are without pedicles and lie as free encapsuled masses in the growing wood.

In many of these conditions, there is a very strong resemblance between these growths and some of the bony exostoses after which they are named. Especially when one breaks them off the trunk of the beech, or the holly, or cedar, and sees their pedicle of attachment and the bark, like integument and periosteum, continued over them, one cannot but compare them with the narrow-based ivory-exostoses of the skull or the pedicled exostoses which are common on the femur and humerus, or with the sessile fibroid uterine tumours.

On section, they show themselves formed of very hard wood; and their pedicles appear as cylinders of wood passing from their centres into continuity with the normal wood of the trunk. Through these pedicles, while they last, the exostoses probably obtain some of their materials

¹ Berkeley in *Gardeners' Chronicle*, 1855 and 1857; Trécul . . . des Loupes et des Broussins in *Ann. des Sc. Nat.*, tome xx. 1853. An excellent account of the knots and veins of wood to which they are related is given by Frank; *l. c.*, *Maserbildung*, p. 394.

for growth; when detached, they wholly subsist and increase on materials derived from the cambium spread out over them. In this continued growth, when encapsuled, they resemble the typical tumours of our pathology more than do any other morbid growths on plants; and they may continue to grow so long as nutritive material is supplied to them.

Now, the history of these growths is very suggestive to us. They are derived from buds, which remain, as Trécul says, in a sort of lethargic state for several years, and then become active, and form either a little branch or a *loupe* or exostosis, which, in its increase, will project more and more beneath the bark. Surely, they may thus confirm that theory of tumours which regards those whose structure does not differ widely from the natural structures as growths derived from portions of germinal substance remaining, though one knows not why, for years 'lethargic,' and then becoming active, growing in their own method, and subsisting on materials derived from the living parts around them.¹

Before I end, let me briefly say some things in apology and self-defence.

If I have seemed to speak as a botanist or any kind of naturalist, I have erred. It is more than forty years since I was a botanist, and any one familiar with the science as it now is may justly say that I know nothing about it. He may detect at once my ignorance even of the language in which I have tried to tell where, in the field of botany, good facts for pathologists may be found.

I have not tried to deal with the whole subject of the

¹ I think that many examples of 'club' on the roots and stems of cabbage and other species and varieties of the same order may illustrate the growth of tumours. But some are certainly insect-galls, and the vegetable-parasitic origin of others is proved.

diseases of plants. It is far wider than I could learn, even if the duty of my life were not to study those of men. I have not tried to learn more from them than may help to give me, and perhaps you, a clearer and wider view of human pathology and the duties it imposes upon us.

In dealing with even this narrow range of the diseases of plants, and in referring to the vastly greater number of those which occur in them—especially in the plants which we, to our own not their advantage, cultivate—I may seem to have been giving evidence against belief in the influence of nervous systems in the production and methods of organic diseases. I have had no such design against any reasonable belief of the kind; I have long held, and often declared, a full belief in the trophic influence of nerve-force. An alteration in the nervous force in a part can surely alter its method of nutrition, even without altering the condition of its blood-vessels, as much as an alteration of its temperature can; and, to cite no others, the facts which Brown-Séquard and Hutchinson brought before us yesterday, tested as they have been, are beyond refutation. Only, let us learn from a pathology more nearly elemental than that which we can study in animals, that many processes of disease, even the most various, may occur where there is no nervous system; and that there are many processes of degeneracy, and of repair, and of disease, of which the essential features are alike whether in the presence or in the absence of a nervous system. The study of such processes in plants, and even in crystals, may help us towards learning the real relation between the nervous force and the processes of organic disease. A certain normal state of nervous force abiding in each part accessible to it may be deemed a necessary condition of the healthy life of that part; as a certain normal temperature is

a necessary condition, and perhaps even a certain electric state. But we are bound to recognise the distinctions between the conditions of life and the essential properties of living things; and, as I proposed when beginning, so now I hope I may have succeeded in showing that it will be very useful if you will study pathology in bodies less complex than our own, especially in such as are not subject to those conditions of our own elemental organic life, which are of all the most difficult to estimate—the conditions, namely, of a nervous system and a quickly circulating and quickly changing blood.

And now let me, only for a minute, claim the position of a senior in our profession, that I may tell how the study may be useful for something more than the direct advancement of knowledge.

I hold it to be very desirable that every one of us should, all his life long, study some science in a scientific manner. There seems to be no equally good method for maintaining the temper and the habits which, by making us always good students, will make us as good practitioners as we can be. There is no method so good for maintaining a constant habit of inquiry, with accuracy and perseverance in research, the power of weighing evidence, of calmly judging, and of accurately speaking; none better for cultivating the love of truth, the contempt for fallacies, whether others' or our own, the gentleness and courtesy which are appropriate to the consciousness of the imperfection of our knowledge.

You may say that we can study science in the practice of our profession. So we may: at least, some of those among us may, who, before getting into practice, spent many years in scientific pursuits, and acquired habits which it is very hard to lose; but, even for these, the study of science in their practice is too much like that which,

I suppose, an astronomer may pursue in some stormy night on a railway journey. For really scientific study, we need repeated observations in unchanged conditions, the right to watch deliberately the course of natural events, sometimes to change that course when and how we please, to set-down our descriptions of the objects of our study even while we watch them, sometimes to have them at home and readily at hand for revision and refreshment of memory. These and many other conditions for scientific study cannot be had in our practice, in which our first and never-ceasing duty must be to do, as soon as we can, all the good we can, though in doing it we may lose or spoil all the best opportunities of acquiring fresh knowledge.

If, then, I am thus far right, that we all are always bound to study a science in a scientific manner, and that it is very hard to do this in our practice, let me again commend to you this elemental pathology. In any fragment of it, studied in those fragments of time which we are all too apt to waste, you may find ample opportunities for observation and experiment, for recording, collecting, and comparing facts, for generalising, for imagining and testing; thus you may gain the happiness and reputation of discovery; and, without ever leaving the paths of duty, you may refresh and strengthen your minds in the study of stores of beauty, wonder, and utility, such as, in all ages, have attracted the wisest and the best of men.

XIV.

THEOLOGY AND SCIENCE.

An Address given at the Leeds Clergy School, December, 1880.

WHEN your Vicar and my son asked me to address you, it seemed to me that, among the very few subjects not too remote from your studies on which I could venture to speak, the best might be the position of antagonism in which theology and science seem to stand. I thought that, having some knowledge of one of these subjects and a profound respect for both, I might be able to show you some of the reasons for this antagonism ; and that, while showing them, the most prudent methods of dealing with it might appear.

It has come to be supposed by many that science in its modern form is always opposed not only to intellectual and inferential theology but to the Christian religion. This supposition is not just. The proportion of scientific men who profess the Christian faith is, I believe, about the same as that of literary men or of lawyers or merchants or any other group of men in the same social position or of equal general culture. You will find among scientific men very few who attack either theology or religion. The attacks imputed to them are made, for the most part, by those who, with a very scanty knowledge of science, use, not its facts, but its most distant inferences, as they do whatever else they can get from any source, for the overthrow of religious beliefs.

Still, it is quite true and need not be doubted that, on many subjects with which both are concerned, theology and science are often at variance; that, of their several beliefs, some seem at present irreconcilable; and that some are held to be so utterly incompatible that if one of them be true, the other must be false. I believe, too, that many who hold by both science and theology are conscious that they do not maintain the truths of both on similar grounds; they adhere to the one with a degree of faith which they would hold to be in the other an unwise credulity. And in this I suppose that practical theologians would think them very right; telling as they do that religion is not wholly a matter of intellectual discernment, not a matter to be classed with all those of science which 'come with observation'; that it is not only with the head but 'with the heart' that 'man believeth.' You may be as sure as you can be of anything of the kind that there are good Christians among those who maintain in science some opinions which some theologians deem dangerous; and perhaps I may be able to show you how this may well be.

The first step towards an explanation of the oppositions of science and theology may be made, I believe, by the admission of a fact which the common affairs of life as well as all history may prove, namely, that in all subjects in which beliefs or opinions are based on inferences from imperfect knowledge, wide differences exist among persons of different mental constitutions or of different mental culture.

I hardly need say that this is the very condition in which the controversies of science and theology are maintained; that they arise, as from time immemorial they have arisen, in inferences from imperfect knowledge.

In science this imperfection is evident to every one who knows anything at all. All scientific men would agree that their increasing knowledge has always brought them to a larger view of the unknown; all would admit the justice of the common illustration that as they have ascended the hill so have they seen, however dimly, more of the distant valleys and streams not yet explored. I suppose it is the same with theologians. You will all say 'we see as in a glass, darkly'; 'we know in part'; that even revelation is incomplete and the understanding of it very limited by the utter imperfection of human language for the expression of the mind of God.

Then, next, let it be observed that, in this general imperfection of knowledge on both sides, the disputes between science and theology almost always begin in questions concerning those very parts of each in which the attainment of nearly certain knowledge is most difficult, or (may it not be said?) impossible. They do not begin on matters of fact or of clear induction; and, though they may lead up to them, they do not begin on subjects on which there has been clear revelation. There is, really, no article of any of the Christian creeds which can be a subject of direct scientific inquiry. Neither do these disputes begin on questions of history or of morality. They begin,—whatever subjects may in a prolonged contest be invaded—they begin in questions on which knowledge is neither clearly revealed nor clearly within the present reach of science; in such questions as the method of creation, the relation of man to the lower animals, the nature and relations of mind and matter, on free-will and law, on the possible nature and conditions of states of conscious existence other than those in which we live. No one can justly maintain that

either revelation or science can supply nearly exact knowledge on these matters, or can make us sure of what may be inferred from what we think on them. Yet, on all of them, both theologians and men of science, and many who are neither, are apt to draw inferences and frame beliefs from what they think to be most likely; and they draw them in different directions; and then their beliefs become so pleasant and seem to fit the facts so well, that each side is ready to hold everything to be untrue which will not fit-in with its own belief. Thus, or by some such course as this, it has come to pass that some, relying on theology, think that, by distant inferences from incomplete revelation, they can assign limits to science; and some, relying on science, suppose that, by as far-reaching deductions from incomplete knowledge, they can prove the fallacy of revelation.

Now, it will be well for you to accept it at once as a certainty that disputes such as these are natural and inevitable; except in the gravity of the consequences to which they may lead, they are only such as occur on all matters in which opinions are given on things concerning which knowledge is incomplete. The students of science and of theology would form a very strange exception from the general rule if they did generally agree on doubtful matters. For, on what doubtful things do men generally agree? on what are they content to doubt? on what do not some decide and then divide into parties mutually aggressive? Surely it has all the generality of a law of our nature that, on all doubtful matters, the vast majority of men refuse to wait for knowledge, and arrange themselves in parties, each of which maintains that itself is sole possessor of the truth. Instances of this rule are everywhere; it is, I say, like a law of our nature; you see it in the great field of imperial politics and in the little

one of parish-vestries ; you see it in literature, in art, in fashions ; you see it everywhere.

It is well to remember this because it may help to show that the antagonism of which I am to speak is not wholly the fault of those engaged in it, or of either of the subjects which they discuss. Still, we might expect that the antagonism between theologians and men of science would be unceasing and very earnest. For it would be safe to assume that, in most instances, men choose the study of the one or the other of these subjects because of some natural inclination implying some mental fitness or disposition for the method of thought required for it. But even if we were to assume, improbable as it may be, that the students of both subjects start all alike and similarly minded, they could not so remain after training so different as theirs. I do not for a moment suggest that one mode of training is absolutely better than the other ; I do not doubt that each is, on the whole, the best for the work to be done after it ; but think how different they are.

There is, indeed, no mental power for which there is not as full range in theology as in science ; none of which some of the best models may not be found among theologians ; but, during pupilage, the general trainings are very different and their influence is rarely lost. The scientific student is encouraged to inquiry, guessing, and testing, the theological to acceptance of acknowledged truth ; the one is assured that knowledge cometh with observation, the other that it does not all so come, and that much is given to him that will do the Will of God ; the one is guided to the exclusion of all sentiment and partiality, the other to such intense desire for the honour of GOD and our Saviour as shall wish everything to be true that can minister to it.

In conditions such as these, it seems inevitable that when any doubtful thing is studied by both theologians and men of science—especially if it be studied by those who are by mind or education chiefly either the one or the other—their conclusions should be different. It is as if two groups of men, placed at stations wide apart, were to look into a dark room containing various strange objects, on which each group could throw from afar some light. The glimmerings of the various forms, lighted from their different lamps, could not seem alike to all; you would not expect the same descriptions from them all, much less the same inferences and reasonings on what they had seen. Similarly, when theologians pure and men of science pure study and reflect on any doubtful matter, it cannot but happen that the opinions of the one group should often seem to the other absolutely wrong and mischievous, wrong and incompatible with the truth.

Thus, then, the existence of these disputes may be explained without assuming or believing in any necessary opposition between the truths of science and religion. And you may hold it for certain that such disputes as these will continue. As knowledge advances, so will discussions be renewed on constantly new questions and on old ones in constantly changing terms; and the discussions will still be sharp, for the temper of opposition to religion will always exist in some persons, and the temper of unreasonable fear of change in others; and, perhaps above all, the disputes will continue, because we love to hold by what we think, for what we think has in it more of ourselves than what we know.

It may even seem likely that, in the future, as knowledge widens and divides its fields, and men's studies become more specialized and distinct, the opposition will become more intense, the deviations wider, the difficulty

of reconciliation greater; for each group will become less and less able to appreciate the works of the others. A learned professor of Tübingen speaking, not long ago, of the progress of knowledge, said that he feared that the temple of science would fail of being finished for the same reason as did the Tower of Babel, because the workmen did not know each other's language. And there is, indeed, great truth in the symbol. There are very few men living who can, I will not say study, but even understand the language of the whole of any recent volume of the *Philosophical Transactions* of the Royal Society.

But on this point the history of science is opposed to what we might expect. As the field of science has been more divided, and studies have been more special, and men have worked on narrower fields, so has the unity of nature become more evident; they have dug deeper and come nearer to a centre. Here is a point which seems to me most worthy of your regard. Let me illustrate it by some instances.

In my early studies it was held by many that Life, or the Vital Principle, that which was deemed the active power in all living things, was not only different from the principles at work in dead matter, but absolutely and essentially opposed to them all. It was thought in some measure profane and irreligious to hold that Life, regarded not as a condition but as a thing, could be in any kind of relation or alliance with anything acting in dead matter, as with chemical affinity, caloric, magnetism or anything of the kind. But, while men have been more and more separating themselves into groups of physiologists, and physicists, and chemists, and each of these again into lesser groups, the intimate relation of all the forces of matter whether living or dead, their correlations and mutual convertibility, have become more and more evident.

Similarly, it was believed, hardly more than half a century ago, that the chemical compositions of organic and of inorganic matter were essentially unlike, and that the organic could not be attained except through operations of a vital power. Now, chemistry makes hundreds of compounds, not distinguishable from those formed in living bodies; and the late researches of M. Friedel, showing that carbon, the most characteristic element of organic compounds, can be replaced in some of them by silicon, one of the most characteristic elements of the inorganic, seem to show that all attempts to indicate a clear line of distinction between the chemistry of the living and that of the dead will fail.

Again, the likeness of things that were deemed diverse is illustrated by Darwin's observations on the carnivorous plants. One used to think that, if there were a sure mark of distinction between plants and animals, it was that these had, and those had not, stomachs with which they could digest, change, and appropriate alien nutritive substances. He has shown as true digestion in plants,—especially by the leaves of the *Drosera*, the little Sun-dew which you may gather on the moors—as can take place in any of our own stomachs; a digestion true, complete and similar to our own.

Yet further, Darwin's last book, on the *Movements of Plants*, makes it more than ever clear that we must think very cautiously in assigning the existence of a nervous system as a really characteristic distinction between plants and animals. So, in respect of diseases, I have lately tried to show that between ours and theirs there is no difference of kind, however much theirs may be, in comparison, free from the complications of nervous system, moving blood, and mind, in which we have to study our own. Nay, even beyond plants; I have

ventured to suggest that a truly elemental pathology must be studied in crystals, after mechanical injuries or other disturbing forces.

I might cite many instances more, but these may suffice for illustration of the general fact, that in the progress of knowledge, while scientific men have seemed to be working more and more widely apart, they have found more and more near relations among all the objects of their study. As the rays of knowledge have extended and diverged, so has their relation to one common centre become more evident, and the unity of nature has become more significant of the unity of God.

Now, I would use these considerations as part of the reasons for inducing you to remember always, that when two beliefs seem incompatible, it does not follow that one is true and the other false; they may both be true. In the disputes of theologians and men of science, it is generally believed that one side must be in the wrong; yet, in many of them, both may be right, and their opposition may be due to their both being ignorant of some intermediate truth which, when gained by increasing knowledge, will combine the truths they now hold apart.

Many true things have been deemed incompatible, yet were all true; they were distant parts of one system of truth; whose common bond was not yet found. I have spoken of the discovered correlation of the vital and the physical forces: I wish I could similarly reconcile the present dispute about the relation of the mental and the physical, but, impossible as such a reconciliation may now seem, I venture to believe that some of you will see it. While physicists are striving in one direction, and metaphysicists in another, and each side is, perhaps, contemptuous of the methods of the other, they will gradually discover truths in the dark interspace between them,

truths which will show not the error of either side, but only the imperfect knowledge of both. Or it may be that, even before this is done, some one mind, with a rare grasp of knowledge, will discern the bond by which their differences may be combined.

If you would look for the reconciliation of truths that seem incompatible, you may find them in the history of discoveries. If the knowledge possessed in any one century could have been declared to the most learned of the men of any century before it, the greater part of it would have been incompatible with their most sure and reasonable beliefs. If any one rubbing amber in the eighteenth century, and seeing it attract and repel a hair, had been told that therein was some of the sure beginning of the knowledge by which, in the nineteenth century, men, thousands of miles apart, would talk with one another, would it not have been called utter folly and incompatible with most certain knowledge? Yet time and constant study have brought reconciliation, not by disproving the truths held in the last century, but by combining them with larger truths.

I remember performing an operation which, only a few years before, would have been intensely painful and attended with profuse bleeding. But, with the help of ether-inhalation and of an instrument then recently invented, it was absolutely painless, and only a few drops of blood were shed. And Sir William Lawrence who was sitting by said, 'If, five years ago, any one had told me that he could do that operation without either pain or blood-shed, I should have been disposed to sign his certificate for Bedlam.'

If, then, large parts of the sure knowledge of one century would have seemed incompatible with knowledge which was as sure in the century before and still remains

sure, it should not be thought strange that some of the knowledge revealed, and revealed, as we may believe, because it is not within the attainment of human power, should seem at present incompatible with the truth which science does attain to : incompatible, not because of error in the interpretation of the revelation, or of error in science, but because of incompleteness in both, and because of the want of some intermediate truths by which both shall be combined.

In this view, I would urge you to maintain, in all these discussions, that both sides are right in that which may be claimed as well-ascertained knowledge ; and that distant inferences on one side should not be allowed to weigh against knowledge or great probability on the other. If it be maintained, as an inference from facts in science, that miracles are impossible, or a resurrection, or that GOD became man, so let it be : from the purely scientific point of view such things seem impossible. But from the religious point of view we may hold them to be not only possible but sure ; and the religious conviction has a right to be not less strong than the scientific. Let, then, each side hold firm by that for which there is clear evidence, whether it be of revelation or of science ; let both wait and work for the intermediate truths that will prove both to be, in the main, right ; let both be aware that the further they carry their inferences from what is known into that which is unknown, the greater is the probability of error and of controversy. Science cannot infer or define all possibilities ; theology cannot interpret all the truths of science ; on doubtful things they are nearly sure to disagree ; they had better wait, not for an untying, but for the tying of the knot which shall combine their many truths in one.

Still, while waiting, there will be many controversies ;

of this you may be sure: and it is greatly to be wished that they should be managed better than they usually are. Let me suggest to you some further means by which you may help to this end.

I have often spoken of the imperfection of both scientific and theological knowledge; but this imperfection is very far from justifying the levity with which the knowledge of one side is sometimes regarded on the other. True, the knowledge of both sides is imperfect and may be thought trivial if we compare the quantity known with what we guess to be the quantity of the unknown; but, in both theology and science the knowledge of the learned is vast if we compare it with that of the unlearned. In one I know it, and in the other I believe it, to be larger than can be compassed by even the strongest intellect in even the longest life. Such knowledge should not be made light of. Neither should the vast labour and vast mental power which have been spent in attaining it. I speak to students of theology; think of the minds that have produced the knowledge of which you study the mere elements; think of their laborious research, their keen logic, their meditations, their earnest and pure love of truth. Ask yourselves, can all this have issued in mere fallacy? Surely not. But, think again; minds as strong, as keen, as earnest, and as truth-loving have been engaged in science; year after year their works accumulate, are discussed, approved. Can they have been wandering in error? or have they only now, at last, gone wrong? or can their conclusions or their very reasonable inferences be justly criticized with less knowledge or less mental discipline than you would think necessary for the criticism of one of your own great masters?

Here seems to me a chief fault of the dispute in the form in which it commonly prevails and makes the most

noise. On each side, many persons, and these not usually of the wisest, think it easy to judge and just to condemn the works of the most learned on the other side. This is, indeed, the way of the world; each man thinks it easy to judge in his neighbour's business. Mr. Darwin, for instance, endowed with a matchless power of observation, with the simplest and purest love of truth, with rare caution and rare power of reasoning on his facts, spends a long life in the study and interpretation of the ways of nature: he submits his facts and his beliefs to the scientific world; they are scrutinized and discussed, and approved by a vast majority of those who are able to judge them; and then some one with no more knowledge of natural history than may be gathered from popular lectures or magazines does not hesitate to speak of these beliefs as absurd, irreverent, subversive of the teaching of the Bible.

On the other side, Dr. Pusey or Dr. Westcott spends year after year in the study of theology, with all the helps of rare literary and linguistic knowledge, with keen analysis, deep reasoning and meditation, and with earnest longing for the truth; and some mere student of science, or one who, at most, has read a little theology at leisure times, thinks himself fit to decide that there cannot be good reasons for any of the beliefs which theologians such as these maintain and teach.

It is worth observing how general is this habit of thinking it much easier to ascertain the truth in another man's business than in one's own. A magistrate, for instance, or any lawyer, would think it iniquitous if it were proposed to decide on any matter of fact in court—such, for instance, as whether a boy did or did not steal a pair of shoes—without examining on oath and thoroughly cross-examining every one supposed to know anything about it: he would accept nothing but what is sworn to;

he would not let the boy be acquitted or convicted unless twelve men, on their oaths and directed by a learned and impartial judge, should agree that the boy did or did not steal those shoes. And yet, as I have often known, the same man will accept as a fact in medical science, as a fact by which he may guide his conduct, any careless statement made by a silly woman: or he will adopt, as a theological principle, the opinion of a poorly educated preacher or his less educated self. Now, his care in the charge of stealing is justified by his jealous fear of doing wrong; it is none too great; it would be well if, in every serious inquiry after truth, no consideration less complete than that of a judicial investigation were deemed sufficient. But the levity with which the same person decides in a matter of science or of theology argues an utterly erroneous estimate of the difficulty of ascertaining truth in other matters than those tried in law-courts; as if the evidences of scientific or of theological beliefs needed to be less, or were less hard to attain, than the evidence whether a certain pair of shoes were stolen by a certain boy.

It would be difficult to overstate the mischiefs of this levity of judgment in grave matters. It is, as I said, the way of the world; but students of truth, whether in theology or in science, should be 'not of the world.'

And yet more, let me venture to say, each side should avoid the habit of thinking that they can safely impute inferences as necessary consequences of the beliefs held by the other; that they can easily show what must come of carrying-out a belief to what they call its logical consequences. It is from this that much of the bitterest part of controversy is derived. It is declared that if this or that probably harmless opinion be allowed, some grievous error or some utter folly must come next. 'It stands to reason,' they say. 'Stands to reason.' One is tempted to ask,

first, whose reason? Is it the reason of a really reasonable man? and of one well instructed in the subject of inquiry? But in any case, it should be remembered how many things that did stand to reason have fallen at the test of fact. I am sure it is true in science, I suspect it is true in theology, that all the beliefs which we now know to have been erroneous, and all the denials of what we now know to have been true, did once 'stand to reason.' They did so stand, with all seeming strength and security, in the minds of those who maintained them and were ready to defend them as certain truths. It stood to reason that the sun moved round the earth, and that people could be bewitched, and that the moon had much to do with lunatics: it stood to reason, even with the rare power of reasoning of Bishop Berkeley, that tar-water would cure and prevent many serious diseases. And I suppose that in every heresy the error has stood to reason in the minds of many who held to it.

There are few expressions which, in serious matters, we should more carefully avoid than this, or any which imply that we can of our own mental power infer certainties, or settle the boundaries of probabilities, or the consequences of beliefs, in subjects which we have not thoroughly studied.

In all temptations to positive assertion in doubtful matters I have found great advantage from keeping in mind an opinion which I think must be just; for it is expressed, though in different terms, by two men of the highest mental power, yet so unlike that together they may express a very wide consent of thought, Mr. Darwin and Bishop Berkeley; the one, so far as I can judge, standing first in power of observing and judging on accumulated facts, the other as high in reasoning on thoughts. They both have held that strong convictions

are not usually evidences of knowledge. Each of them says it after reflecting on the imperfection of knowledge in his own subject. How clearly and constantly should it be remembered by those who are ready to hold and to act upon strong convictions in their criticisms of the results or supposed consequences of other men's studies!

On this habit of being positive in doubtful things, of which the worst form is that of being positive in doubtful things which others have studied much more and in more appropriate manners than ourselves, let me recommend to you some sentences by Canon Mozley, who, more than any whose works I have read, seemed to combine the best powers of the theologian and the man of science. They are in the beginning of the conclusions with which he sums up his work on the *Augustinian Doctrine of Predestination*,—a work from which one of the clearest lessons is that, even in theology, there are truths which seem incompatible with one another, and yet must be held patiently till, in future knowledge, they may be reconciled. That which may be regarded as the text on which he here writes is his first sentence, 'It were to be wished that that active penetration and close and acute attention which mankind have applied to so many subjects of knowledge, and so successfully, had been applied in somewhat greater proportion than it has been, to the due apprehension of that very important article of knowledge, their own ignorance.' Let me advise you to study his exposition of this text, even though you may never need to use it in other than purely theological studies.

But now, I suspect that after I have talked so long of our imperfect knowledge, ignorance and doubts, you may be ready to ask, Is doubtfulness to be a constant disposition of our minds? Is doubt always to be encouraged in others?

And to this I suppose that all prudent men would say, on many things 'Yes,' on some things 'No.' There are some certainties in science which may be deemed unalterable, though the methods of expressing them may change; and many more truths which future knowledge may modify and embody in larger truths but will not contradict. These things must be held absolutely, and they who raise doubts on them need not be listened to or answered. And so in theology, and in the Christian faith which it expounds; there are not only clear evidences which, in their accumulated force, cannot, I think, be reasonably resisted by those who will fairly collect and try them; but there are convictions of religious faith, not always based on knowledge, or on other evidence than the faith which is 'the evidence of things unseen,' which may justly be held as unalterable, because they are consistent with revelation, and have been sustained by the testimony of clouds of witnesses, and, I believe, have in many minds the testimony of God's indwelling Spirit.

It would be by complete wisdom if all the truths on both sides could be enumerated, and all the probabilities arranged in just order of merit; such wisdom would be more than human, though the tone of some controversialists may imply that it is not beyond the reach of very ordinary intellects. I only undertook to speak to you of some of the reasons for the antagonism between theology and science, and of the disposition with which you may regard it. Of the truths and highest probabilities on both sides, I will only express my belief that they may justly be held together, though on different grounds; that they are not within reach of direct mutual attack; that those of one side cannot be directed against those of the other except by far-reaching inferences; by inferences of which the probability is in an inverse proportion to their distance

from the truths or the reasonable beliefs on the one side or the other.

How, then, should you regard the conflict?

First, do not think of the controversies as if they were an unmixed evil. Evil they may be to some engaged in them; but they have the worth of that system of conflict and competition, out of which some of our highest good has come. Man has reached his present state in civilized races through an incessant struggle not only for food and life, but for intellectual mastery; for virtue as against those vices that are only brutality surviving; for truth as against error. The influences of Christianity and of civilization have made the struggle more gentle; the better sort of men do not destroy one another; but the law of conflict is not abrogated. The struggle which, from age to age, has ensured the survival of the fittest, has been under a law which includes intellectual conflicts and has constantly helped to the attainment of the truth.

I cannot doubt that, in theology, the thoroughness and clearness with which the truth has been and is maintained is due, in great measure, to the mental power exercised and provoked by controversy: and surely the conflict, unceasing as it has been, has added a weighty fact to the evidence of Christianity. I doubt whether there be any instance of a fallacy which has resisted for centuries the force of controversy among people of the same race and of high mental power. Fallacies have existed much longer and more widely than Christianity; but they have existed only when not closely examined by persons well-trained and willing to expose them, or in weak or untrained minds incapable of discerning truth and error. Witchcraft, for instance, and many like fallacies, were long and widely believed in; but a century or less sufficed to dispel the belief in all intelligent minds,

as soon as the grounds on which it rested were explored by men willing to doubt and able to observe and think. Christianity, from its very beginning to this day, has been maintained and disputed by some of the keenest intellects of the most cultivated races; yet it stands firm. And if, as is often said, it was never so assailed as in the last half century, it has assuredly in that same time attained a marvellous growth among all classes. The mere fact of its continued existence among intelligent people is a weighty evidence of its truth.

Thus, you may regard these controversies with, at least, much patience. Whether you need to engage in them is not for me to say. If you do, let me advise you to remember always that there is nothing in the nature of the study of science to make those who pursue it more generally averse from religion than others of the same mental power. Among them, as among those engaged in literature, in art, in law, are men of various mental character, who are variously affected by the same evidence, variously by the same appeals to sentiment, variously by each of all the arguments that theology can use. Some men of science cannot accept religious doctrine on any evidence other than that which would suffice to establish a general law of nature; some cannot but believe that all which Christians refer to God's upholding and informing power is within the range of mere natural laws; some, not doubtful of the great central truths of religion, hesitate to accept a few or many of the theological inferences from them; some are, or might be, clerics, holding as fully as any that the whole teaching of the Church is true. In being thus various, scientific men are only, as other groups are, variously influenced by the same evidences.

It is, therefore, not against scientific men, or science

itself, that you can need to contend ; it must be, if at all, against inferences derived from scientific knowledge or belief, and used as arguments against religion. And, if you decide to contend, you should bind yourselves to a real study in science. Study it in any department that you like. [I resist a great temptation to try to convert you all to the belief that science is a far more delightful pursuit than theology can be.] But in whatever part you choose, study it by observation, by experiment, by collecting, as well as by reading. And let your reading be in the works of the best masters, that you may learn their true spirit, their strength, their methods of observing and thinking, their accuracy in describing. No mere light or popular study of science can teach you this, or enable you to discern what in it is true, what is more or less probable, what is mere inference, what is mere guess. The more you learn, the more you will be able to think how much remains unknown ; the more you will feel and be able to show the improbability that all that is now unknown can ever be comprised within the terms, however much extended, of the natural laws already known. It is in trying to think of the unknown that one may best feel confident that future discoveries, both in science and in the interpretation of revelation, will bring to men the knowledge by which beliefs, now seeming incompatible, will be reconciled.

Think, with all the imagination that you can, of the crowd of things unknown ; the possible forces not yet discerned, or as little guessed at as was the electric force three centuries ago ; think of the possible new forms or properties of matter ; of that fourth dimension which, I believe, some can conceive ; think of our absolute relation in continuity with the whole world of that which transmits light, and of the real nature of which I suppose that

scarcely more is known than that it is not nothing. May we not be sure that the study of these things unknown will in time, however distant, discover truths which, if they could now be told, would seem incompatible with many that appear most sure, but which, when they shall be discerned, and set in order, and grouped with those derived from the continued study of theology, will fill the intervals by which our beliefs now stand apart? For future knowledge will not be merely heaped-up on the surface of that we now possess; it will penetrate the mass and fill its gaps and interspaces, and make many things one which as yet seem multiple and alien. This has always been the course of knowledge—a growth not by accretion, but interstitial, compacting, unifying. And surely, as the growth of knowledge has harmonized many truths that seemed in discord, so will it attain to the clear evidences of the unity of theology and science.

XV.

PRESIDENTIAL ADDRESS AT THE OPENING OF THE
INTERNATIONAL MEDICAL CONGRESS, 1881.

THE words which have been graciously spoken by his Royal Highness the Prince of Wales—a prince beloved of all, because he loves to promote the happiness of all—make it unnecessary to defend the meeting of an International Congress. Let me only suggest to you some thoughts as to the work which, being in Congress, we have to do, and the spirit in which it may best be done, so that the good effects of our meeting may last long after our parting.

In the largest view of our design, it may seem to be that of bringing together a multitude of various minds for the promotion and diffusion of knowledge in the whole science and art of medicine, in their widest range, in all their narrowest divisions, in all their manifold utilities. And this design, I cannot doubt, will be fulfilled; for although the programme tells of selected subjects for discussion, and defines the order of our work, yet knowledge will be promoted in a much wider range in the meetings without order, which will be held every day and everywhere—meetings of men with all kinds of mental power, and all forms of knowledge and of skill; every one ready alike to impart and to acquire knowledge.

It is safe to say that in the casual conversations of this coming week there will be a larger interchange and

diffusion of information than in any equal time and space in the whole past history of medicine. And with this interchange will be a larger increase, for in the mart of knowledge he that receives gains, and he that gives retains, and none suffer loss.

The increase will be the greater because of the great variety of minds which will meet. As I look round this hall, my admiration is moved, not only by the number and total power of the minds which are here, but by their diversity; a diversity in which I believe they fairly represent the whole of those who are engaged in the cultivation of our science. For here are minds representing the distinctive characters of all the most gifted and most educated nations; characters still distinctly national, in spite of the constantly increasing intercourse of the nations. And from many of these nations we have both elder and younger men; thoughtful men and practical; men of fact and men of imagination; some confident, some sceptic; various, also, in education, in purpose and mode of study, in disposition and in power. And scarcely less various are the places and all the circumstances in which those who are here have collected and have been using their knowledge. For I think that our calling is pre-eminent in its range of opportunities for scientific study. It is not only that the pure science of human life may match with the largest of the natural sciences in the complexity of its subject-matter; not only that the living human body is, in both its material and its indwelling forces, the most complex thing yet known; but that in our practical duties this most complex thing is presented to us in an almost infinite multiformity. For in practice we are occupied, not with a type and pattern of the human nature, but with all its varieties in all classes of men, of every age and every occupation, in all climates

and all social states; we have to study men singly and in multitudes, in poverty and in wealth, in wise and unwise living, in health and all the varieties of disease; and we have to learn, or at least to try to learn, the results of all these conditions of life while, in successive generations and in the mingling of families, they are heaped together, confused, and always changing. In every one of all these conditions man, in mind and body, must be studied by us; and every one of them offers some different problems for inquiry and solution. Wherever our duty or our scientific curiosity, or, in happy combination, both, may lead us, there are the materials and there the opportunities for separate original research.

Now, from these various opportunities of study, men are here in Congress. Surely, whatever a multitude and diversity of minds can, in a few days, do for the promotion of knowledge, may be done here. Every one has something he may teach, much more that he may learn; and, in the midst of apparent utter confusion, knowledge will increase and multiply. It has been said, indeed, that truth is more likely to emerge from error than from confusion, and, in some instances, this is true; but much of what we call confusion is only the order of nature not yet discerned; and so it may be here. Certainly, it is from what seems like the confusion of successive meetings such as this that that kind of truth emerges which is among the best moving and directing forces in the scientific as well as in the social life—the truth which is told in the steady growth of general opinion.

But it is not proposed to leave the work of the Congress to what would seem like chances and disorder, good as the result might be; nor yet to the personal influences by which we may all be made fitter for work, though these may be very potent. In the stir and controversy of

meetings such as we shall have, there cannot fail to be useful emulation; by the examples that will appear of success in research, many will be moved to more enthusiasm, many to more keen study of the truth; our range of work will be made wider, and we shall gain that greater interest in each other's views and that clearer apprehension of them which are always attained by personal acquaintance and by memories of association in pleasure as well as in work. But as it will not be left to chance, so neither will sentiment have to fulfil the chief duties of the Congress.

Following the good example of our predecessors, certain subjects have been selected which will be chiefly, though not exclusively, discussed, and the discussions are to be in the sections into which we shall soon divide.

Of these subjects it would not be for me to speak even if I were competent to do so; unless I may say that they are so numerous and complete that—together with the opening addresses of the Presidents of Sections—they leave me nothing but such generalities as may seem commonplace. They have been selected, after the custom of former meetings, from the most stirring and practical questions of the day; they are those which chiefly occupy men's minds, and on which there is at this time good reason to expect progress, or even a just decision, from very wide discussion. They will be discussed by those most learned in them, and in many instances by those who have spent months or years in studying them, and who now offer their work for criticism and judgment.

I will only observe that the subjects selected in every section involve questions in the solution of which all the varieties of mind and knowledge of which I have spoken may find their use. For there are questions, not only on many subjects, but in all stages of progress towards

settlement. In some the chief need seems to be the collection of facts well observed by many persons. I say by many, not only because many facts are wanted, but because in all difficult research it is well that each apparent fact should be observed by many; for things are not what they appear to each one mind. In that which each man believes that he observes, there is something of himself; and for certainty, even on matters of fact, we often need the agreement of many minds, that the personal element of each may be counteracted. And much more is this necessary in the consideration of the many questions which are to be decided by discussing the several values of admitted facts and of probabilities, and of the conclusions drawn from them. For on questions such as these minds of all kinds may be well employed. Here, there will be occasion even for those which are not unconditionally praiseworthy, such as those that habitually doubt, and those to whom the invention of arguments is more pleasing than the mere search for truth. Nay, we may be able to observe the utility even of error. We may not, indeed, wish for a prevalence of errors; they are not more desirable than are the crime and misery which evoke charity. And yet in a Congress we may palliate them, for we may see how, as we often read in history, errors, like doubts and contrary pleadings, serve to bring out the truth, to make it express itself in clearest terms, and show its whole strength and value. Adversity is an excellent school for truth as well as for virtue.

But that which I would chiefly note, in relation to the great variety of minds which are here, is that it is characteristic of that mental pliancy and readiness for variation which is essential to all scientific progress, and which a great International Congress may illustrate and promote. In all the subjects for discussion we look for

the attainment of some novelty and change in knowledge or belief; and after every such change there must ensue a change in some of the conditions of thinking and of working. Now for all these changes minds need be pliant and quick to adjust themselves. For all progressive science there must be minds that are young whatever may be their age.

Just as the discovery of auscultation brought to us the necessity for a refined cultivation of the sense of hearing, which was before of only the same use in medicine as in the common business of life; or, as the employment of the numerical method in estimating the value of facts required that minds should be able to record and think in ways previously unused; or, as the acceptance of the doctrine of evolution has changed the course of thinking in whole departments of science; so is it, in less measure, in every less advance of knowledge. All such advances change the circumstances of the mental life, and minds that cannot, or will not, adjust themselves, become less useful, or must, at least, modify their manner of utility. They may continue to be the best defenders of what is true; they may strengthen and expand the truth, and may apply it in practice with all the advantages of experience; they may thus secure the possessions of science and use them well; but they will not increase them.

It is with minds as with living bodies. One of their chief powers is in their self-adjustment to the varying conditions in which they have to live. Generally, those species are the strongest and most abiding that can thrive in the widest range of climate and of food. And, of all the races of men, they are the mightiest and most noble who are, or by self-adjustment can become, most fit for all the new conditions of existence in which by various

changes they may be placed. These are they who prosper in great changes of their social state; who, in successive generations, grow stronger by the production of a population so various that some are fitted to each of all the conditions of material and mode of life which they can discover or invent. These are most prosperous in the highest civilization; these whom Nature adapts to the products of their own arts.

Or, among other groups, the mightiest are those who are strong alike on land and sea; who can explore and colonise, and in every climate can replenish the earth and subdue it; and this not by tenacity or mere robustness, but rather by pliancy and the production of varieties fit to abide and increase in all the various conditions of the world around.

Now, it is by no distant analogy that we trace the likeness between these, in their successive contests with the material conditions of life, and those who are to succeed in the intellectual strife with the difficulties of science and of art. There must be minds which in variety may match with all the varieties of the subject-matters, and minds which, at once or in swift succession, can be adjusted to all the increasing and changing modes of thought and work.

Such are the minds we need; or rather, such are the minds we have; and these in great meetings prove and augment their worth. Happily, the natural increase in the variety of minds in all cultivated races is—whether as cause or as consequence—nearly proportionate to the increasing variety of knowledge. And it has become proverbial, and is nearly true in science and art, as it is in commerce and in national life, that, whatever work is to be done, men are found, or soon produced, who are exactly fit to do it.

But it need not be denied that, in the possession of this first and chiefest power for the increase of knowledge, there is a source of weakness. In works done by dissimilar and independent minds, dispersed in different fields of study, or only gathered into self-assorted groups, there is apt to be discord and great waste of power. There is, therefore, need that the workers should from time to time be brought to some consent and unity of purpose; that they should have opportunity for conference and mutual criticism, for mutual help and the tests of free discussion. This it is which, on the largest scale and most effectually, our Congress may achieve; not, indeed, by striving after a useless and happily impossible uniformity of mind or method, but by diminishing the lesser evil of waste and discord which is attached to the far greater good of diversity and independence. Now, as in numbers and variety the Congress may represent the whole multitude of workers everywhere dispersed; so in its gathering and concord it may represent a common consent that, though we may be far apart and different, yet our work is and shall be essentially one; in all its parts mutually dependent, mutually helpful; in no part complete or self-sufficient. We may thus declare that, as we who are many are met to be members of one body, so our work for science shall be one, though manifold; that as we, who are of many nations, will, for a time, forget our nationalities, and will even repress our patriotisms, unless for the promotion of a friendly rivalry; so will we in our work, whether here and now, or everywhere and always, have one end and one design—the promotion of the whole science and whole art of healing.

It may seem to be a denial of this declaration of unity that, after this general meeting, we shall separate into

sections more numerous than in any former Congress. Let me speak of these sections to defend them ; for some maintain that, even in such a division of studies as these may encourage, there is a mischievous dispersion of forces. The science of medicine, which used to be praised as one and indivisible, is broken-up, they say, among specialists, who work in conflict rather than in concert, and with mutual distrust more than mutual help.

But let it be observed that the sections which we have instituted are only some of those which are already recognized, in many countries, in separate societies, each of which has its own place and rules of self-government and its own literature. And the division has taken place naturally in the course of events which could not be hindered. For the partial separation of medicine, first from the other natural sciences, and now into sections of its own, has been due to the increase of knowledge being far greater than the increase of individual mental power.

I do not doubt that the average mental power constantly increases in the successive generations of all well-trained peoples ; but it does not increase so fast as knowledge does, and thus, in every science, as well as in our own, a small portion of the whole sum of knowledge has become as much as even a large mind can hold and duly cultivate. Many of us must, for practical life, have a fair acquaintance with many parts of our science, but none can hold it all ; and for complete knowledge, or for research, or for safely thinking-out beyond what is known, no one can hope for success unless by limiting himself within the few divisions of the science for which, by nature or by education, he is best fitted. Thus, our division into sections is only an instance of that division of labour which, in every prosperous nation, we see in

every field of active life, and which is always justified by more work better done.

Moreover, it cannot be said that in any of our sections there is not enough for a full strong mind to do. If any one will doubt this, let him try his own strength in the discussion of several of them.

In truth, the fault of specialism is not in narrowness, but in the shallowness and the belief in self-sufficiency with which it is apt to be associated. If the field of any speciality in science be narrow, it can be dug deeply. In science, as in mining, a very narrow shaft, if only it be carried deep enough, may reach the richest stores of wealth and find use for all the appliances of scientific art. Not in medicine alone, but in every department of knowledge, some of the grandest results of research and of learning, broad and deep, are to be found in monographs on subjects that, to the common mind, seemed small and trivial.

And study in a Congress such as this may be a useful remedy for self-sufficiency. Here every group may find a rare occasion, not only for an opportune assertion of the supreme excellence of its own range and mode of study, but for the observation of the work of every other. Each section may show that its own facts must be deemed sure, and that by them every suggestion from without must be tested: but each may learn to doubt every inference of its own which is not consistent with the facts or reasonable beliefs of others; each may observe how much there is in the knowledge of others which should be mingled with its own; and the sum of all may be the wholesome conviction of all, that we cannot justly estimate the value of a doctrine in one part of our science, till it has been tried in many, or in all.

We were taught this in our schools; and many of us have taught that all the parts of medical science are

necessary to the education of the complete practitioner. In the independence of later life, some of us seem too ready to believe that the parts we severally choose may be self-sufficient, and that what others are learning cannot much concern us. A fair study of the whole work of the Congress may convince us of the fallacy of this belief. We may see that the test of truth in every part must be in the patient and impartial trial of its adjustment with what is true in every other. All perfect organizations bear this test; all parts of the whole body of scientific truth should be tried by it.

Moreover, I would not, from a scientific point of view, admit any estimate of the comparative importance of the several divisions of our science, however widely they may differ in their present utilities. And this I would think right, not only because my office as President binds me to a strict impartiality and to the claim of freedom of research for all, but because we are very imperfect judges of the whole value of any knowledge, or even of single facts. For every fact in science, wherever gathered, has not only a present value, which we may be able to estimate, but a living and germinal power of which none can guess the issue.

It would be difficult to think of anything that seemed less likely to acquire practical utility than those researches of the few naturalists who, from Leeuwenhoeck to Ehrenberg, studied the most minute of living things, the Vibrionidæ. Men boasting themselves as practical might ask, 'What good can come of it?' (I am speaking in the presence of M. Pasteur.) Time and scientific industry have answered, 'This good: those researches have given a more true form to one of the most important doctrines of organic chemistry; they have introduced a great beneficial change in the most practical part of surgery;

they are leading to one as great in the practice of medicine ; they concern the highest interests of agriculture, and their power is not yet exhausted.'

And as practical men were, in this instance, incompetent judges of the value of scientific facts, so were men of science at fault when they missed the discovery of anæsthetics. Year after year the influences of laughing-gas and of ether were shown : the one fell to the level of the wonders displayed by itinerant lecturers, students made fun with the other ; they were the merest practical men, men looking for nothing but what might be straight-way useful, who made the great discovery which has borne fruit not only in the mitigation of suffering, but in a wide range of physiological science.

The history of science has many similar facts, and they may teach that any man will be both wise and dutiful if he will patiently and thoughtfully do the best he can in the field of work in which, whether by choice or chance, his lot is cast. There let him, at least, search for truth, reflect on it, and record it accurately ; let him imitate that accuracy and completeness of which I think we may boast that we have, in the descriptions of the human body, the highest instance yet attained in any branch of knowledge. Truth so recorded cannot remain barren.

In thus speaking of the value of careful observation and records of facts, I seem to be in agreement with the officers of all the sections ; for without any intended consent, they have all proposed such subjects for discussion as can be decided only by well-collected facts and fair direct inductions from them. There are no questions on theories or mere doctrine. This, I am sure, may be ascribed, not to any disregard of the value of good reasoning, or of reasonable hypotheses, but partly to the

just belief that such things are ill-suited for discussion in large meetings, and partly to the fact that we have no great opponent schools, no great parties named after leaders or leading doctrines about which we are in the habit of disputing. In every section the discussions are to be on definite questions, which, even if they be associated with theory or general doctrines, may yet be soon brought to the test of fact; there is to be no use of doctrinal touchstones.

I am speaking of no science but our own. I do not doubt that in others there is advantage in dogma, or in the guidance of a central organizing power, or in divisions and conflicting parties. But in the medical sciences I believe that the existence of parties founded on dominant theories has always been injurious; a sign of satisfaction with plausible errors, or with knowledge which was even for the time imperfect. Such parties used to exist, and the personal histories of their leaders are some of the most attractive parts of the history of medicine: but, although in some instances an enthusiasm for the master-mind may have stirred a few men to unusual industry, yet very soon the disciples seem to have been fascinated by the distinctive doctrine, content to bear its name and to cease from active scientific work. The dominance of doctrine has promoted the habit of inference, and repressed that of careful observation and induction. It has encouraged that fallacy to which we are all too prone, that we have at length reached an elevated sure position on which we may rest, and only think and guide. In this way specialism in doctrine, or in method of study, has hindered the progress of science more than the specialism which has attached itself to the study of one organ, or of one method of practice. This kind of specialism may enslave inferior minds: the specialism of doctrine can enchant

into mere dreaming those that should be strong and alert in the work of free research.

I speak the more earnestly of this because it may be said, If our Congress be representative, as it surely is, may we not legislate? May we not declare some general principles which may be used as tests and as guides for future study? We had better not.

The best work of an International Congress is in the clearing and strengthening of the knowledge of realities; in bringing, year after year, all its force of numbers and varieties of minds to press forward the demonstration and diffusion of truth as nearly to completion as may from year to year be possible. Thus, chiefly, our Congress may maintain and invigorate the life of our science. And the progress of science must be as that of life. It sounds well to speak of the temple of science, and of building and crowning the edifice. But the body of science is not as any dead thing of human work, however beautiful; it is as something living, capable of development and a better growth in every part. And as in all life the attainment of the highest condition is only possible through the timely passing-by of the less good, that it may be replaced by the better, so is it in science. As time passes, that which seemed true and was very good becomes relatively imperfect truth, and the truth more nearly perfect takes its place.

We may read the history of the progress of truth in science as a palæontology. Many things which, as we look far back, appear like errors, monstrous and uncouth creatures, were, in their time, good and useful, as good as possible. They were the lower and less perfect forms of truth which, amid the floods and stifling atmospheres of error, still survived; and, just as each successive condition of the organic world was necessary to the

evolution of the next following higher state, so from these were slowly evolved the better forms of truth which we now hold.

This thought of the likeness between the progress of scientific truth and the history of organic life may give us all the better courage in a work which we cannot hope to complete, and in which we see continual and sometimes disheartening change. It is, at least, full of comfort to those of us who are growing old. We that can read in memory the history of half a century might look back with shame and deep regret at the imperfections of our early knowledge, if we might not be sure that we held, and sometimes helped onward, the best things that were, in their time, possible, and that they were necessary steps to the better present, even as the present is to the still better future. Yes—to the far better future; for there is no course of nature more certain than is the upward progress of science. We may seem to move in circles, but they are the circles of a constantly ascending spiral; we may seem to sway from side to side, but it is only as on a steep ascent which must be climbed in zig-zag.

What may be the knowledge of the future none can guess. If we could conceive a limit to the total sum of mental power which will be possessed by future multitudes of well-instructed men, yet could we not conceive a limit to the discovery of the properties of materials which they will bend to their service. We may find the limit of the power of our unaided limbs and senses; but we cannot guess at a limit to the means by which they may be assisted, or to the invention of instruments which will become only a little more separate from our mental selves than are the outer sense-organs with which we are constructed.

In the certainty of this progress, the great question

for us is, What shall we contribute to it? It will not be easy to match the recent past. The advance of medical knowledge within one's memory is amazing, whether reckoned in the wonders and precision of the science not yet applied, or in its practical results in the general lengthening of life, or, which is still better, in the prevention and decrease of pain and misery, and in the increase of working power. I cannot count or recount all that in this time has been done; and I suppose there are very few, if any, who can justly tell whether the progress of medicine has been equal to that of any other great branch of knowledge during the same time. I believe it has been; I know that the same rate of progress cannot be maintained without the constant and wise work of thousands of good intellects; and the mere maintenance of the same rate is not enough, for the rate of the progress of science should constantly increase. That in the last fifty years was at least twice as great as that in the previous fifty. What will it be in the next; or, for a more useful question, What, I repeat, shall we contribute to it?

I have no right to prescribe for more than this week. In this let us do heartily the proper work of the Congress, teaching, learning, discussing, looking for new lines for research, planning for mutual help. It will be hard work if we will do it well; but we have not met for mere amusement or for recreation, though for that I hope you will find fair provision, and enjoy it the better for the work preceding it.

And when we part let us bear away with us, not only much more knowledge than we came with, but some of the lessons for our conduct in the future which we may learn in reflecting on the work of our Congress.

In the number and intensity of the questions brought before us, we may see something of our responsibility.

If we could gather into thought the amounts of misery or happiness, of helplessness or of power for work, which may depend on the answers to all the questions that will come before us, this might be a measure of our responsibility. But we cannot count it; let us imagine it; we cannot even in imagination exaggerate it. Let us bear it always in our minds, and remind ourselves that, great as it may be now, our responsibility will constantly increase. For, as men become in the best sense better educated, and the influence of scientific knowledge on their moral and social state increases, so, among all sciences there is none of which the influence and, therefore, the responsibility will increase more than ours; because none more intimately concerns man's happiness and working power.

But, more clearly in the recollections of the Congress, we may be reminded that in our science there may be, or, rather, there really is, a complete community of interests among men of all nations. On all the questions before us we can differ, discuss, dispute, and stand in earnest rivalry; but all consistently with friendship, all with readiness to wait patiently till more knowledge shall decide which is in the right. Let us resolutely hold to this when we are apart: let our internationality be a clear abiding sentiment, to be, as now, declared and celebrated at appointed times, but never to be forgotten; we may, perhaps, help to gain a new honour for science if we thus suggest that in many more things, if they were as deeply and dispassionately studied, there might be found the same complete identity of international interests as in ours.

And then, let us always remind ourselves of the nobility of our calling. I dare to claim for it, that among all the sciences, ours, in the pursuit and use of truth,

offers the most complete and constant union of those three qualities which have the greatest charm for pure and active minds—novelty, utility, and charity. These three, which are sometimes in so lamentable a disunion, as in the attractions of novelty without either utility or charity, are in our researches so combined that, unless by force or wilful wrong, they hardly can be put asunder. And each of them is admirable in its kind. For in every search for truth we can not only exercise curiosity, and have the delight—the really elemental happiness—of watching the unveiling of a mystery, but, on the way to truth, if we look well round us, we shall see that we are passing among wonders more than the eye or mind can fully apprehend. And as one of the perfections of nature is that, in all her works, wonder is harmonised with utility, so is it with our science. In every truth attained there is utility either at hand or among the certainties of the future. And this utility is not selfish: it is not in any degree correlative with money-making; it may generally be estimated in the welfare of others better than in our own. Some of us may, indeed, make money and grow rich; but many of those that minister even to the follies and vices of mankind can make much more than we. In all things costly and vainglorious they would far surpass us if we would compete with them. We had better not compete where wealth is the highest evidence of success; we can compete with the world in the nobler ambition of being counted among the learned and the good who strive to make the future better and happier than the past. And to this we shall attain if we will remind ourselves that, as in every pursuit of knowledge there is the charm of novelty, and in every attainment of truth utility, so in every use of it there may be charity. I do not mean only the charity which is in hospitals or in the service of the

poor, great as is the privilege of our calling in that we may be its chief ministers; but that wider charity which is practised in a constant sympathy and gentleness, in patience and self-devotion. And it is surely fair to hold that, as in every search for knowledge we may strengthen our intellectual power, so in every practical employment of it we may, if we will, improve our moral nature; we may obey the whole law of Christian love, we may illustrate the highest induction of scientific philanthropy.

Let us, then, resolve to devote ourselves to the promotion of the whole science, art, and charity of medicine. Let this resolve be to us as a vow of brotherhood; and may God help us in our work.

XVI

THE CONTRAST OF TEMPERANCE WITH ABSTINENCE.

An Essay republished from the 'Contemporary Review,' 1881, by the kind permission of the Editor.

IT may be assumed that there is no need now to write such arguments against intemperance as would be required if it were defended by any reasonable sober person. All reasonable people hold it to be a hideous evil, and few know more of its mischiefs than do surgeons, who see its baneful influence in multiplying the injuries due to accident and violence, and in hugely increasing the danger and mortality of operations and of injuries such as sober people bear with impunity.

The arguments against intemperance are complete and unanswerable, and in favour or defence of it there are none. But the necessity of total abstinence from alcoholic drinks is now, as for a great national advantage, urged on those who are habitually moderate with nearly as much vehemence as on the habitual drunkards. It is said by some that there is not any habitual use of alcoholic drinks which is not imprudent, even if it may not be called intemperate, and that even in small quantities they are always and to all healthy persons injurious, slowly, it may be, but surely. And some, who see in them no direct harm, yet maintain that they are useless and unnecessary, and ought to be disused, so that, by overwhelming examples and custom of total abstinence, the crime and folly

of intemperance may be put down. Statements such as these are confidently made; but if we look for evidence there seems to be very little in favour of them, and there is more that inclines the other way. The whole of the evidence, indeed, which has as yet been collected for a comparison of the respective influences of temperance and of abstinence on nations or on large bodies of men, may seem not sufficient for a complete final decision. The subject is a very large one and very complicated; and, though the central question may seem narrow, it is involved in so many more that the final general answer must be subject to exceptions for particular cases, only to be settled by many future and very careful researches. Still, on the whole, and on the question of national health and strength, I cannot doubt, with such evidence as we have, that the habitual moderate use of alcoholic drinks is generally beneficial, and that in the question raised between temperance and abstinence the verdict should be in favour of temperance.

The evidence of the evils of intemperance is abundant, clear, and complete. If any one accustomed to weigh facts will compare with it what is given as the evidence of the evils of a moderate use of alcohol, this must appear as, at the best, quite insignificant. Against intemperance we have the vast experience of life-assurance offices, the records of large hospitals, the unanimous opinion of all practitioners of medicine, the results of all physiological studies, the belief of all reasonable persons. Against moderation we have none of these.

So far as I know, no large insurance-office or general hospital has yet begun to collect facts for statistics bearing on the respective influences of moderation and of abstinence. Their records are of the 'temperate' and the 'intemperate' in various degrees; if they have any records

of the 'total abstainers,' they are far too few for any useful comparison. And it is hard to see how in any of these institutions sufficient statistics can be gained for a decision of the general influences of habits so little apart, and practised under so great variety of conditions, as are moderation and abstinence. The least that could be used with any chance of getting at the truth would be a careful comparison of five hundred total abstainers who have never been intemperate, and were not born of intemperate parents, with five hundred habitually moderate persons similarly born and bred, pursuing similar callings, and living under generally similar conditions; and this comparison should have regard not only to average length of life, and to health at different periods of life, but to the quantity of muscular work and of good mental work done by each group. With less evidence than such comparison as this might supply, I cannot suppose that any statistics can be worth using in the question between moderation and abstinence. The reports of the health of prisoners, and of the quantities of work done by them, whilst both total abstinence and work are compulsory, cannot safely be used. Two sets of conditions can hardly be more unlike than those in a prison and those out of it. In the prison every arrangement is made for the maintenance of health and of fitness for work; every arrangement must be submitted to as absolutely as total abstinence; every prisoner's day's work must be done. But we cannot so much as guess what would be the health and what the power and kind of work of the same prisoners if, when free, they could do as they please in everything but total abstinence. And what is true of prisoners is, in this view, equally true of the inmates of workhouses.

Thus we have no statistics, and are not within reach

of any, for deciding the question between moderation and abstinence.

As for the opinions of the medical profession, they are, by a vast majority, in favour of moderation. It may be admitted that, of late years, the number of cases has increased, in which habitual abstinence from alcoholic drinks has been deemed even better than habitual moderation. But, excluding those of children and young persons, the number of these cases is still very small, and few of them have been observed through a long course of years, so as to test the probable influence of a life-long habitual abstinence. Whatever weight, then, may be assigned to the balance of opinions among medical men, it certainly must be given in favour of moderation, not of abstinence.

Then, we have some deductions from physiological observations which are supposed to indicate a mischief in even habitual moderation. But some of these are really such that if, in the place of 'alcohol,' we were to read 'common salt,' we should be led to conclude, if it were not for experience to the contrary, that we are destroying ourselves by the daily excessive use of a material which, in its excess, can alter the constitution of our blood, or the permeability or other properties of our tissues. And even the best of the physiological observations on alcohol do not touch the question between abstinence and moderation more nearly than as suggesting some of the directions which further inquiries should take. Medical science has always been full of facts derived from physiology; facts which seemed certainly good guides to practice, safe signals of what must be true for training in health or for remedy of disease. Some of these have so proved themselves, and have led to some of the best knowledge we possess; but many more have proved fallacious; and I suppose that all who are actively engaged in practice are

sure that experience alone can be trusted for deciding the practical value of a deduction from physiology. Till we have this experience, large and clear, we must not regard the facts of physiology concerning alcohol as more than reasonable suggestions, facts, or opinions, to be received with all respect, but to be practically tested before they can be regarded as practically useful, or as decisive of the question now discussed.

The beliefs of reasonable people are, doubtless, by a large majority favourable to moderation rather than abstinence, and this should not be regarded as of no weight in the discussion. For, although the subject be one in which few even among reasonable people have made any careful observations, and fewer still have thought with any care, yet this very indifference to the subject, this readiness to fall-in with custom, a custom maintained in the midst of a constant love of change, and outliving all that mere fashion has sustained,—all this is enough to prove that the evidence of the custom being a bad one is not clear.

Thus, then, from all the witnesses to the evils of intemperance we fail to get any clear evidence that there is mischief in moderation. Looking further we find in them certain indications that it is, on the whole, generally beneficial. The long-abiding custom of which I just now spoke makes this very probable. The use of alcohol, and, speaking generally, its habitually moderate use, has been for many centuries the custom of a large majority of civilised nations. We may safely say that there is a natural disposition among adult men to drink; a natural taste for alcoholic drinks, whether for their cheering influence or for any other reason. In the absence of any clear evidence to the contrary there must be a presumption that such a natural taste has its purpose rather for

good than for evil. In a general view of the natural tastes of all creatures for foods and drinks, we see that tastes are guides to good and not to evil. There may be an exception in our own case in relation to alcoholic drinks; there may have been a universal fallacy in the minds of even reasonable, virtuous, and self-denying people, who have believed that in the moderate use of wine or beer they gratified a natural and useful desire; but the evidence of such a mistake ought to be very clear.

For when it is said that the taste for these drinks is not natural, but artificial or acquired, there is error as to what in this case 'natural' should mean. Our natural state is that in which we now live; the present state is to each race of men, if not to every one man, the natural state; the state attained in the natural course of development. In this state men are disposed to drink alcoholic liquids, and the presumption must be that these drinks are beneficially adjusted to some of the conditions of our life which have been attained in our development from some less civilised or completely savage state. Considering how largely our nature has been changed from that state by the gradual developments of society, and by the various habits, dispositions, and capacities therewith associated, it is in the highest degree probable that, with these changes, we should have beneficial adjustments of different foods or other means of sustaining us in our work. Among these we may reckon the greater part of the comforts and of what now seem to be the necessities of our civilised, that is, our natural state, such as wheaten bread, potatoes, cultivated fruits, and well-fed meats; and, similarly, among them we may reckon, unless there be clear reason to the contrary, such things as tea, coffee, and alcoholic drinks, and, I even venture to think, tobacco, though, probably, for only much smaller groups of men.

Doubtless all these things are used by some persons in a mischievous excess; but so may be bread and cheese; and many persons, complying with custom or fashion, may use them to whom even in moderation they are useless or mischievous; but the fact of general or nearly universal custom is, in a matter of this kind, very weighty; it gives a strong presumption in favour of the belief that these are beneficially adjusted to natural necessities. And the presumption is equally strong, whether we regard the matter from the view of natural theology, and think of these things as providential insertions in a divinely guided course of nature, or from the view of natural history, where they may appear as instances in which the gradually developing human mind discerned, though it may have been almost unconsciously, the things fittest for its maintenance and progress. All our foods have thus been provided or chosen; they have suited men's tastes, and therefore have been desired and used, without any clear knowledge of their utility and fitness. Science has ascertained the reasons for their fitness; in the same course it may, in time, make us more sure of the fitness of the moderate use of alcohol.

The presumption in favour of moderation being generally better than abstinence is strengthened by a comparison very broadly made between those of our race who do not and those who do habitually use alcoholic drinks. In this view we may broadly compare the Eastern with the Western races, adding to these the North Americans, Canadians, and Australians. We may again take, as fair tests of the comparative influences of moderation and of abstinence, the average length of life, average health, the quantity of muscular work, and the quantity of mental work. For the first two we have, I think, no evidence that can be relied on; but, at least, there is no reason to

suppose that the Easterns live longer or are healthier than the Westerns and their descendants. But as to working power, whether bodily or mental, there can be no question that the advantage is on the side of those who use alcoholic drinks. And it is advantage of this kind which is most to be desired. Longevity is not the only or the best test of the value of the things on which we live. It may be only a long old age, or a long course of years of idleness or dulness, useless alike to the individual and the race. That which is most to be desired is a national power and will for good working and good thinking, and a long duration of the period of life fittest for these; and facts show that these are more nearly attained by the peoples that drink alcohol than by those who do not. It may not be positively asserted that the alcohol does this good; it may be due to many other things; but in trying to account for it the influence of alcohol must not be excluded or counted as evil.

We find a similar result in a comparison of the races of Europe, among whom different proportions of alcoholic drinks are habitually consumed. Comparing North with South we certainly compare those who drink more with those who drink less; and the advantage is with those who drink more, especially when we compare them in respect of general activity and force of mind and body, in readiness and fitness for work, in enterprise, invention, production, and all the signs of the best mental activity and strength. Doubtless, in all these cases, the result may depend more on other conditions than on the use of alcohol; possibly, it may be even in spite of alcohol; but this is neither proved nor probable, and we have no right to imagine it.

It is to be observed that, in all these comparisons, the case in favour of moderation is burdened by the inclusion

of the intemperate among the moderate. If the shortened lives and damaged healths, the idleness and bad work, of the drunkards, and all the miseries entailed upon their children, could be excluded from the reckoning, the evidence in favour of alcohol would be very greatly strengthened, and the reasons for preferring moderation to abstinence might seem conclusive.

We are bound, further, to consider the great length of time during which the nations compared have followed their different habits in regard to drinking. Let us count it—for the sake of a round number—at a thousand years, a time much below the truth. Knowing as we do the mischiefs that are transmitted through inheritance from the intemperate, it is hardly conceivable that, if moderation were in any degree mischievous, its evils should not by this time have become very evident. The accumulated evils of thirty generations of men given to moderate drinking should now be notable; they should have risen to the level of the manifest evils of one or two generations of excess, or, if they were not positively distinct, they should have appeared in a comparison of the heirs of these drinking generations with the heirs of thirty generations of abstainers. But the result is the reverse of this. West against East, North against South, the heirs of the moderate drinkers are better men in force of body and mind than are the heirs of the abstainers. I say, of moderate drinkers. Some would say that before our time there have been centuries of habitual vile intemperance, and that our ancestors, for so long as we have records of them, drank hideously hard. I do not believe this. We have tales of the intemperance as of all the other extravagances of former times, and, as usual, the more customary moderation is not recorded, being not remarkable. But the worse that is said of our ancestors the less does any

probability of harm, and the greater does the probability of good, from alcohol appear.

It may be worth pointing out how great would be the force of any hereditary evil accumulated in a succession of many generations. Blackstone says, 'It is, at the first view, astonishing to consider the number of lineal ancestors which every man hath within no very great number of degrees; and so many different bloods is a man said to contain in his veins as he hath lineal ancestors. Of these, he hath two in the first ascending degree, his own parents; he hath four in the second, the parents of his father and the parents of his mother; he hath eight in the third, the parents of his two grandfathers and two grandmothers; and by the same rule of progression he hath an hundred and twenty-eight in the seventh, a thousand and twenty-four in the tenth, and, at the twentieth degree, or the distance of twenty generations, every man hath above a million of ancestors, as common arithmetic will demonstrate.'¹

This calculation is not quite accurate, for it neglects the cases of marriages of cousins and of other blood-relations. A person who is the offspring of first cousins may count only six great-grandparents; but, allowing for these and all such cases, we may safely estimate that at 'the distance of twenty generations every man hath' many more than half a million of ancestors: and the estimate of 'above a million,' though inaccurate for the number of ancestors, is necessarily accurate for the number of times of transmission of hereditary properties, and of the converging lines along which, in twenty generations, they must meet on every one born in the twenty-first. Let us then suppose that the moderate use of alcoholic drinks is, in even a very small degree, mischievous, and

¹ Commentaries. Ed. Sweet, 1844. Vol. ii. p. 202.

that the evil due to it is in some degree transmissible, as are those of intemperance, by inheritance : what should be the condition of every one among us, seeing that on each of us some measure of evil must have come along each of more than a million lines with constantly accumulating and convergent force ? It may be said that there were, probably, many total abstainers among the half-million or more of ancestors, but, as probably, there were many drunkards ; and these two groups, each of which we may be nearly sure were a small minority of the whole number, may balance one another, and leave us free to think of the influence of several hundreds of thousands of transmissions of whatever evils can be transmitted from the effects of habitual moderation. I think it would be difficult to find a healthy family born of three successive generations of drunkards, or of persons all suffering with the same heritable disease. If, then, healthy families are born after thirty generations of habitual drinkers of alcohol, how can we fairly charge its moderate use with doing mischief ? Is it not fair to think it probable that it has rather been beneficial, and one among the conditions to which we owe the still gradually increasing healthiness and working power of our race ? At least it must seem clear that the effects of excess, and those of moderation, in the use of alcoholic drinks are so incomparably different in degree, that they may be reckoned as different in kind ; and that though the one is always injurious, the other may be always harmless, and often or usually beneficial.

It is, perhaps, due to the want of a sufficient appreciation of the different effects of the same substance, or the same force, applied in different quantities to the living body, that a moderate use of alcoholic drinks appears to some persons to be necessarily mischievous. If a large

quantity does great harm, a smaller quantity, it is thought, must do some harm, however much less it may be. But the facts are the other way. Large quantities of quinine will make a man, at least for a time, deaf and blind; smaller quantities may do nothing of the kind, but may cure his ague; and yet smaller may leave him with his ague and all his senses unaffected, but may improve his appetite. One quantity of arsenic may kill any man; another much smaller may strengthen him, or cure his neuralgia, or some disease of his skin. Or a Styrian may (perhaps) be all the better for taking, in a year, as much arsenic as, if he took it in a day, would be fatal to him. And the same thing is to be seen in other conditions, though less plainly than in the use of poisons and medicines. The best way to make muscles large and strong is by moderate exercise gradually increased; by the same exercises in excess they may be weakened and brought to waste: the nerves of sight and hearing may be improved by moderate and graduated exercises, ruined by excess of similar exercises: our cuticle may be made to grow thick and strong by duly adjusted friction, and may be spoiled or detached by similar friction not duly adjusted. And the rule is very general: the same influences which in one degree are destructive, are, in a less degree, beneficial. It would only be consistent with this rule that alcohol should be, in large quantities, injurious, in small quantities useful.

I have dealt with the question between temperance and abstinence entirely from the side from which my profession has enabled me to study it so far as may justify my giving an opinion on it. My study makes me as sure as I would ever venture to be on any such question, that there is not yet any evidence nearly sufficient to make it probable that a moderate habitual use of alcoholic drinks is generally, or even to many persons, injurious; and that there

are sufficient reasons for believing that such an habitual use is, on the whole and generally, beneficial. It may be assumed that further study of the matter, by competent and calmly minded scientific persons, will discover many facts concerning the use of alcohol which will lead to the remedy of such harm as, even in moderation, it may do to some persons, or to some whole races of men, and to its use being better directed and limited than in our present customs. But knowledge of this kind will not change the general conclusion in favour of the general utility of a moderate use of alcoholic drinks; and till this knowledge is gained every one may assume that he may safely use them in such moderation as he does not find to be injurious.

But, as I have said, there are many who, even if they would admit this, would yet maintain that the mischiefs of intemperance are so much greater than any conceivable advantages of moderation, that we ought not to promote or defend moderation, because its promotion hinders the general adoption of total abstinence, which, they say, is the necessary and only sure remedy for intemperance. Here, I can only doubt. I should think that in this, as in other things lawful yet tempting to excess, the discipline of moderation is better than the discipline of abstinence. But it is certain that we have no facts at all by which to estimate whether the whole benefits of moderation, or the whole possible benefits or evils of total abstinence, or the whole sure evils of intemperance, would be the greater; we have nothing from which we may make even a fair guess, or which would justify a great experiment. Without such knowledge it seems unreasonable to urge the discontinuance of a custom which is certainly pleasant and probably useful: and very unreasonable to require temperate persons, who are an immense majority of the

population, to cease to do that which is lawful, useful, and agreeable, in order that the intemperate minority may be induced to cease to do that which is unlawful and mischievous. It would be not less unreasonable to urge that honest people should cease to gain money because there are some misers, thieves, and swindlers.

But some will say, What is this moderation? How may we define it? Let those who thus ask try to define, to the satisfaction of any ten persons, what, under all circumstances and to all people, is moderation in bread or the wearing of jewels, in hunting or the language of controversy.

XVII.

EXPERIMENTS ON ANIMALS.

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by the kind permission of the Editor.*

It seems fair to demand that those who inflict pain or other distress on animals, for the purpose of acquiring knowledge, should be judged by the same rules as those who, for any other purposes, do the same. The rules by which these are judged may be read in the customs by which a very great majority of sensible and humane persons encourage or permit the infliction of pain and death on large numbers of animals, for purposes far short of great utility, necessity, or self-defence. It seems in these customs an admitted rule that, for the sake of certain quantities of utility or pleasure, or both, men may inflict great pain on animals without incurring the blame of cruelty. Can it be shown, for those who make painful scientific experiments, that the pain of their experiments is less and the utility more than in the majority of the practices permitted or encouraged by the great majority of reasonable and humane persons among the educated classes in this country?

In enumerating some of the instances of pain-giving which are generally and, as I think, for the most part rightly allowed, I am aware that some may seem trivial, and some nearly necessary to human welfare; however, they are not cited for the purpose of speaking ill of them,

but as examples of practices which, not being deemed blameworthy or restrained by law (unless in respect of the seasons in which they are allowed), may serve as measures with which to compare the pain-giving experiments of scientific enquirers.

Among such practices are the painful restraint and training of our horses and other domestic animals; the caging of birds for the sake of their beauty or their song; the imprisonment of animals of all kinds in zoological gardens and aquaria for study or for amusement. In all these instances animals are compelled or restrained from the happiness of natural life; they have to endure what might be inflicted as severe punishment on criminals—slavery or imprisonment for life. But the inflictions are justified by the utility which men derive from them.

In another large group of painful customs generally encouraged are those inflicting death and often great suffering on birds and beasts for obtaining ornamental fur or feathers; the mutilation of sheep and oxen for the sake of their better or quicker fattening; the multiplication of pains and deaths in the killing of small birds and small fish, such as larks, quails, whitebait, and the like; although, so far as mere sustenance of life is concerned, any weight of food in one large fish or one large bird would serve as well as an equal weight in a hundred small ones. Still, the pleasure of delicious food, or of beautiful decoration, or, in some instances, the utility of better nutriment, seems sufficient to a vast majority of civilised men and women to justify these customs.

In another group may be named all the pain-giving sports—shooting, hunting, stalking, fishing, and the rest—various in the pleasure that they give, various in utility. And in yet another, the trapping, hunting, and killing of mice, rats, stoats, frogs, and toads invading cultivated

land—worms, and slugs, and the whole class of what we call vermin—creatures generally troublesome and sometimes injurious.

From a list such as this, which might easily be enlarged, a rough estimate may be formed of the quantity of pain or distress, imprisonment or death, which, in the opinion of great majorities of persons entitled to judge, may be inflicted on animals for purposes of utility or of pleasure, or from other motives far less than those of necessary self-defence or maintenance of human life. The list may thus serve as a standard with which to compare the pains and the utilities of vivisections. Doubtless many persons would find in it some practices which they would forbid; some would hunt or shoot, but would not keep parrots or larks in confinement; some would eat whitebait or small birds, and wear sealskin, and order the destruction (anyhow) of all the rats and mice in their houses, but would put down fox-hunting and salmon-fishing. But there are very few, even among the generally most sensible and humane, who do not allow or encourage, even if they do not practise, many things of which I think it certain that the pain is greater and the utility less than that of many experiments on living animals. They may do it thoughtlessly, but they may find that they do it, if they will make a careful survey of their furniture, clothes, and ornaments, their food, amusements, and habits of life for a year, and then estimate the pains which in providing all these have been inflicted upon animals. Let them estimate them, if they can, with the same measure as that with which they estimate the pain of vivisection.

Such an estimate will probably seem the more easy the less the subject of pain has been studied. If we reflect on the evidence on which we believe that, from any given injury or disease, anyone must suffer less or

more pain, we find that we are generally guessing, or saying to ourselves, 'It must be so,' without any clear evidence that 'It is so.' At most, if we have ourselves had any injury or disease, we may believe that another in the same condition would suffer just as we did. But few beliefs would be more fallacious. The sensibility to pain is as various as is the 'ear for music'; the disease which by one is described, and very truly according to that one's sensations, as a source of agony only to be compared with the rack or some such torture, is by another described as not very distressing; and the accounts given of it by others imply that between these extremes there are all intermediate degrees. To those who study them in surgical practice it is sure that degrees of pain depend on differences of personality much more than on different intensities or quantities of disease or injury. And there are abundant cases to prove that the general sensibility to pain is far greater among the more than among the less cultivated races of mankind; that savages, as they are called, endure with comparative indifference inflictions which to most persons of the higher races would be terrible. Mental cultivation continued through many generations has not only increased the general keenness of our senses, so that we discern far wider and minuter varieties and combinations of form, colour, sound, and flavour, than can be discriminated by lower races; it seems to have increased equally our sensibility to pain and our power of directing our attention to it. This seems to be especially true among persons with poetic and artistic minds: and, as we may be sure of the contrast between the higher and the lower races of men, so we may believe that the contrast must be yet greater between ourselves and any of the animals lower than man. It is as nearly certain as anything of the kind can be that with every

degree of diminution of the proportion which the nervous system bears to the rest of the body there is an equal diminution of the sensibility to pain—the lower in the scale of nature the less the sensibility; so the pain inflicted by a deer-stalker, a salmon-fisher, or a vivisector is certainly less than would be inflicted in a similar injury on any man.

But the question is whether vivisections inflict less or more pain than do sports or any other generally encouraged pain-giving practices, on animals of the same kind. I may offer some evidence on this question; for while studying and teaching physiology I saw many experiments on animals, and made some; and although I have not seen much of any pain-giving sport, or other such pursuits, yet I have seen enough to enable me to compare the pains they give with those of vivisections, and I have been able to study the effects of wounds with which hares, birds, and other game or vermin have escaped, and have lived long before they were again shot or died, diseased or starved. And for comparison, so far as may be possible, of the pain of injuries and their consequences in animals with those in men, I can call to mind the impression made by hundreds of surgical operations which I saw before the discovery of anæsthetics, and by the thousands of patients watched after operations before and since that discovery.

Of course the pains given in experiments on animals, not under an anæsthetic, are as various as were those which before 1846 were given in surgical operations. But, for the worst, I think it probable that the pain inflicted in such experiments as I saw done by Magendie was greater than that caused by any generally permitted sport; it was as bad as that which I saw given to horses in a bull-fight, or which I suppose to have been given

in dog-fighting or bear-baiting. I never saw anything in his or any other experiments more horrible than is shown in many of Snyders's boar-hunts, or in Landseer's 'Death of the Otter.' Among the most painful experiments I saw many years ago were some for studying the effects of mineral poisons by giving them to animals, but they only matched the poisoning of rats and other vermin, which is encouraged by all who thus direct their destruction. I have never seen or read of an experiment on a fish so painful as the ligger-fishing I have seen, in which a live fish is impaled as bait on a long double-hook, with which he swims till a pike or other big fish swallows him with the hook, and thus remains in his turn hooked all night, till the fisherman unhooks and kills him. I doubt whether any experiment on fish or reptile can in an equal time give more pain than is given in long 'playing' a deeply-hooked salmon, or in any length of time give more pain than is endured by a fish which escapes with a hook fixed deeply in his throat. Probably, a thoroughly heartless vivisector, if one could be found, might inflict in a day more pain than a heartless sportsman; but in the ordinary practice of experiments on animals it is not possible that a vivisector should in a day or a month cause nearly so much pain as would, in the same time, be caused by an active sportsman shooting among abundant game, or a fly-fisherman in a well-stocked stream, or as a man successfully hunting seals or ermines, or poisoning rats. All the vivisectors in Paris will hardly be the cause of nearly so much suffering as the promoters of the scheme for preserves of lions and other great carnivora, to be shot at, in Algeria.

I believe, therefore, that, with these few exceptions which I have mentioned, there are no physiological experiments which are not matched or far surpassed in

painfulness by common practices permitted or encouraged by the most sensible and humane persons of the time.

In this comparison I have been considering experiments in which anæsthetics are not used. Where these are used, as for many years past they have been in the vast majority of experiments, at least in this country, the case is immensely stronger. For, in respect of all these instances of giving pain, there are two distinct things to be estimated—the immediate pain of the inflicted injury, and the consequent pain and other misery, if the injury be survived. As to the first, what has been already said may suffice; it is abolished by anæsthetics; and as to the second, the comparison is more easy, because it may be made between animals injured in vivisection or in any other manner, and men after accidental injuries or surgical operations. When, for this comparison, I call to mind the conditions which I have seen in animals living after vivisection, and those which I have seen in others who have survived the injuries inflicted in sport or in other attempts to kill them, I can only think of them as equal in pain or disability; but with this advantage to the vivisected animal, that it has been an object of care, provided with food and rest, and safe from the attacks of others of its own or other kinds.

I am aware that some say that this keeping alive is itself a shameful cruelty; but probably the animals themselves, if they could think like men, would not so judge; for the vast majority of animals used for experiments are taken from those already marked for death: stray dogs who would be carried away by the police, horses assigned to the knacker, rabbits and guinea-pigs whom none would keep, rats and mice whom anyone would kill or direct to be killed. They may be compared with men dying of some mortal disease, whose lives may

be prolonged by operations which will leave them in some way mutilated, saved from dying, but remaining invalids. In these cases the great majority of persons endure the mutilation for the sake of the longer life, and they very rarely repent their choice; rather, as time passes, and they become habituated to the consequences of the operation, they regain nearly all the happiness of their former healthy life. No one accustomed to such cases can doubt that, if an animal consigned to death should reason as a man, he would accept his life on condition of submitting to any experiment under an anæsthetic. I have seen many animals after vivisection looking as happy as before it; many of them were happier, being better fed and in every way more cared for than they had ever been before.

If it may thus be justly held that the pain and other miseries inflicted by vivisection are less than those inflicted in many practices encouraged by sensible and humane persons, it may next be considered whether their utility be as great. It might justly be asked whether their utility and pleasure be as great, for it will not be denied that pleasure is a considerable motive in most of the sports, and in the wearing of decorative dresses such as cannot be procured without giving pain. But I would rather not argue that man's pleasure can ever be reason enough for his giving pain. It seems impossible to define even nearly the 'when,' or the how much pain for how much pleasure. But, if any will hold the contrary, and that in the pursuit of pleasure pain may be inflicted, even without considerations of probable utility, then it may certainly be maintained that there are no pleasures more intense than the pursuit of new knowledge, nor any for which, if for any, greater pain might be given.

But, omitting the pleasures of both, may the utilities

of the two groups of pain-giving pursuits be estimated? Looking back at the list, it is clear that one method of utility cannot be pleaded for all. Sports may well be justified by the skill, patience, self-control, and endurance which may be trained in them; by the recreation which they provide for tired men; by their great social advantages; by their satisfaction of a desire which, in many minds, has the force of a natural instinct that cannot justly be repressed. As for the restraints, and imprisonments, and fattenings of animals, their utility is in most instances so evident that the whole course of quiet social life and trade would need to be changed if they were forbidden. Besides, for most of these, as well as for most field sports, the creatures would have no opportunity of living at all if it were not given on condition of their submitting to restraint or death at the will of men. There would be no more foxes than wolves in this country if they were not kept to be hunted; pheasants, partridges, and other game would soon be extinct if they were not preserved on purpose to be shot.

The destruction of vermin has, no doubt, utility—is sometimes even necessary for the safety of our food and property; but one must regret that it is so often pursued in a very merciless manner—left to cats or dogs, or slowly-acting poisons, or starvings in traps. The procuring of decorative furs and other parts of dress and furniture, attended as it often is with great suffering to the creatures hunted, may, I suppose, be justified by some utility. But I am not a fair judge of it. I can speak more certainly of the utility of vivisection.

Speaking generally, it is certain that there are few portions of useful medical knowledge to which experiments on animals have not contributed. The knowledge may be now so familiar that the sources from which parts

of it were derived may be forgotten; or what was first found by experiments may now have other evidences; or experiments may only have made sure that which, without them, was believed: but the whole history of medicine would show that, whatever useful or accurate knowledge we possess, we owe some parts of it to experiments on animals.

To different parts of knowledge they have contributed very different proportions; and it is often difficult to assign to them their just proportion. They have never been the sole means of study. Chemists, physicists, practitioners, all have worked as well as physiologists; and the work of each has guided and strengthened that of others. The whole value of experiments on animals, therefore, cannot be estimated by a few examples; it may be made evident in them, and they may indicate how it stands alone in the utility of saving life; but no one can measure it who is not able to analyse the whole progress of medical knowledge during at least the last century.

A clear instance of its utility may be found in the tying of arteries, whether for the cure of aneurism or for the stopping of bleeding. Before Hunter's time—that is, about a hundred years ago—it is nearly certain that ninety-five out of a hundred persons who had aneurism of the principal artery of a lower limb died of it: a few more may have been saved by amputation above the knee, but at that time about half the patients who submitted to that operation died. At the present time, it is as certain that of a hundred persons with the same disease less than ten die. At that time all patients with aneurisms of arteries between the thigh and spine or in the neck or arm-pit, died, unless by some strange course of the disease one or two in a hundred went on living. Now, among all such patients, from fifty to

seventy in every hundred are saved by operations. In the same time there has been a great diminution in the deaths from bleeding after large operations : I remember when such bleeding might be called common ; it is now very rare.

By these improvements in surgery some hundreds of lives are annually saved in this kingdom ; lives of which it may be deemed certain that, less than a century ago, 90 per cent. would have been lost. The proportion saved has from the beginning almost steadily increased, chiefly because of improvements in the materials used for tying the arteries, for which experiments on animals have given good guidance. Hunter and his first followers used tape, or applied extra ligatures in fear that the chief ones should give way, or they put pads under ligatures in fear that the arteries should be cut through. Even then they saved some lives, but many of their patients died. It was a great advance when the changes really produced in arteries variously tied were more exactly ascertained by experiments, especially by those of Dr. J. F. D. Jones in 1805. They showed that single ligatures of twine or silk were better than any others then known, and, using these, the proportion of lives saved by operation was greatly increased. But the ends of these ligatures hanging out of the wounds hindered their healing, and sometimes excited such irritation that the tied arteries were ulcerated, and, with losses of blood, the patients died. Many things were tried in animals and men ; precautions constantly more careful were taken ; various silks and various twines and wires were used, with very slowly increasing success, till (omitting some facts in the history of progress) catgut was employed. This could be left in the wound, and the skin could be closed over it and quickly healed. It was a great improvement, and has certainly saved many lives which even ten years ago

would have been lost. Still, in spite of catgut, specially manufactured and carefully carbolised and used with every precaution, some few patients die, and some operations fail through defects in the ligatures. Now, it seems probable that catgut may be superseded by thread prepared from sinews of the kangaroo.

Such is a mere sketch of the progress by which a disease which less than ninety years ago was fatal to at least 90 per cent. of those affected with it is now fatal to not more than 10 per cent. If we add to this the great diminution in the losses of life from wounds of arteries, whether they be wounded in accidents or in operations—a diminution similarly due, for the most part, to the improvement of ligatures—it is safe to say that not less than five hundred lives are now saved every year, in this country alone, which fifty years ago would have perished. In this, as in every case, all methods of study have been used: careful watching of the patients, examinations of the dead, published records of failures as well as of successes, experiments on animals; and it is not possible to assign exactly to each of these its share in the good result; but no one who can fairly judge after many years' active practice of surgery will doubt, I think, that at least one-fifth may be assigned as the share due to experiments on animals—say, at least, one hundred lives a year in this one department of surgery.

Histories similar to these may be told of the improvement of many other parts of surgical practice, through knowledge of the processes of repair, as in fractures, divided tendons, divided nerves, or as in the union of separated parts and in grafting. In these there may rarely be questions of saving life; but in all of them, the length of illness and the degree, if any, of permanent impairment of power for work or pleasure depends in great

measure on the knowledge of facts and principles to which experiments on animals have contributed. But I need not dwell on these ; others have already abundantly illustrated them ;¹ I will rather suggest some general considerations on the whole subject.

Looking back over the improvements of practical medicine and surgery during my own observation of them in nearly fifty years, I see great numbers of means effectual for the saving of lives and for the detection, prevention, or quicker remedy of diseases and physical disabilities, all obtained by means of knowledge to the acquirement or safe use of which experiments on animals have contributed. There is scarcely an operation in surgery of which the mortality is now more than half as great as it was forty years ago ; scarcely a serious injury of which the consequences are more than half as serious ; several diseases are remediable which used to be nearly always fatal ; potent medicines have been introduced and safely used ; altogether such a quantity of life and of working power has been saved by lately-acquired knowledge as is truly past counting. And in these advantages our domestic animals have had due share by the improvement of veterinary medicine. What proportion is due to experiments on animals no one can tell ; it would be as easy to estimate the proportion contributed by each national means of education to the general intellectual improvement of our population. Let it be guessed at a tenth or a twentieth of the whole, and in either case the

¹ I may refer to the evidence of Sir William Gull, Dr. Burdon Sanderson, Professor Turner, and Professor McKendrick before the Royal Commission of 1876 ; to the addresses of Mr. Bowman to the British Medical Association at Chester ; the 'Memorandum' issued by the late Professor Sharkey and other physiologists during the passage of the late Act through Parliament ; the address to the Surgical Society of Ireland by Professor MacDonnell, and the remarkable lectures recently given before the International Medical Congress.

utility of vivisection must far surpass that of the great majority of pain-giving practices permitted or encouraged by thousands of persons of recognised humanity and good sense. And it is by these, when duly informed on the facts, that the question should be judged, for it is eminently one of those in which sentiment is predominant on one side, reason on the other; in which the arguments on one side are mainly based on kindly feeling and sympathy with sufferings of which the amount is guessed at, while on the other they rest mainly on facts observed, on considerations of utility, and on the desire for knowledge. The only competent judges in such a case are those in whom sentiment and intellectual power are fairly balanced, and who will dispassionately study the facts and compare the pain-giving and the utility of experiments on animals with those of any generally allowed or encouraged pursuit.

But it may be said, Would not all this useful knowledge have been gained by the other methods of study, without the experiments; less quickly, perhaps, but not less surely? And now will not scientific progress be as sure, though not so speedy, without as with them? Possibly, yes; most probably, no. But suppose it were so, what should we say to those who suffer by the delay? At the present time 20,000 persons are annually killed by venomous snakes in India. If the discovery of a remedy without experiments on animals would come later by, say, five years, than one made with their help, would it be nothing to have lost 100,000 lives? The case is worth considering because of an almost absurd consequence of the Vivisection Act. I may pay a rat-catcher to destroy all the rats in my house with any poison that he pleases; but I may not myself, unless with a licence from the Home Secretary, poison them with snake-

poison, nor, without an additional certificate, try to keep them alive after it.

Happily for other people, medical practitioners who watch the course of science are not content to wait longer than can be helped; they see the miseries of disease and of all its consequences better than others do, and they are stirred to the desire of knowledge by motives of the greatest force—by humanity and the consciousness of deep responsibility; for they are just as plainly bound to acquire more knowledge as they are to use aright that which is already at hand. Moreover, they are stirred by emulation and a fair ambition of success, and the unhappiness of failures in what had full promise of good. No distress, I think, can be greater than that of losing a life committed to one's charge by some accident, as it must be called, which might have been averted by some piece of knowledge which seems within reach. Such are the losses of life in the use of anæsthetics.

The annual deaths from chloroform in this country used to be about twenty. They were, probably, not more than 1 in every 30,000 of persons to whom it was given; but they were intolerable to those who felt in any measure responsible, though blameless, for them, and many went back to the use of ether, which is safer, though less convenient. Probably less than 1 in 50,000 die of it; but I saw one die to whom it was faultlessly given, and he was so good and generous a man that I felt it would have been right to kill a hundred animals either to save his life or to find out why he died, and to be able in the future to avert so awful a catastrophe.

It is in reflection on cases such as this, in which lives are lost or health is impaired for want of knowledge which seems to be within reach of experimental study, that medical men of science feel justly impatient of the

restraints put upon these researches. They know that such knowledge as they want has often been gained by experiments on animals ; they know that the experiments made in this country are, both severally and in their total, far less pain-giving and far more useful than are either the shooting, hunting, or fishing practised by many, and encouraged by nearly all, of the best people in the land ; they see all round them mere luxuries of dress and furniture, gathered at immense cost of pain and misery, and perhaps only a little more useful than might be obtained from animals killed for necessary food ; and yet they find themselves selected for legislative restraint and still exposed to public and private charges of vile cruelty, abused in sensational meetings, and as much as possible hindered in the studies which even legislation would permit.

Of course, among the opponents of experiments on animals there are several very different groups, and with some of these it is useless to appeal to reason. Some have committed themselves to the agitation, and cannot recede without dispute or more material loss, and some are carried on with so strong an impulse of a mind once made up that they cannot pause for a revision of their judgment. But there are many who favour the agitation only because they are ignorant of the facts of the case ; they have heard or read accusations of cruelty grossly mis-stated, and have heard no defence or denial of them.

That which is most to be desired is that persons with fairly-balanced minds, with at least an average both of humanity and of capacity for judgment in cases in which deep feeling may be stirred, should study the whole matter, and judge of experiments on animals as they would of other practices in which utility or even pleasure is pleaded as justifying the infliction of pain. Let them visit physiological laboratories, and see what is done, and

compare the work and its results with those of a day's shooting, or a night's trapping of rabbits, or of any sport or trade in which the lives of animals are concerned.

And chiefly it is to be wished that the subject should thus be thoroughly studied by those who administer the Act. If they would thus study it, they would be more sure that the Act is at least a sufficient deference to public sentiment, they would use the discretion allowed them in its administration, so as to throw as little obstacle as possible in the way of competent persons engaged in the most useful and beneficial inquiries, and they would resist further restraints of experiments on animals with as much resolution as they resist other hindrances to the doing of what they judge to be right.

XVIII.

SOME RARE AND NEW DISEASES.

The Bradshawe Lecture, given at the Royal College of Surgeons of England 1882. Its sub-title is 'Suggestions for the Study of Part of the Natural History of Disease.'

MR. PRESIDENT AND GENTLEMEN,—It is my first duty, in delivering the first Bradshawe lecture in our college, to offer a tribute of respectful thanks to the generous lady by whom it was founded, the widow of Mr. William Wood Bradshawe, a Fellow of this college, who practised at Andover and at Reading and died in 1866. He was a home-loving and studious man, who diligently cultivated his mind in both literature and science; and his widow, who survived him fourteen years, being desirous to testify her gratitude for the happiness which she owed to him, bequeathed a thousand pounds to this college, and as much to the Royal College of Physicians, on the condition that each should institute a lecture, to be given annually and to bear his name. She desired that the lecture should be on some subject connected with medicine or surgery, and that the choice of the lecturer should rest with the president of each college for the time being. She made no stringent regulations, and seems to have wished only to maintain her husband's name in good repute, by associating it with the advancement of the science which he loved.

In my endeavour to fulfil her exemplary wish, I have

chosen the subject of 'Some Rare and New Diseases.' I hope to be able, in speaking of them, to illustrate a part of the natural history of disease which I think is too little studied—that part, namely, which relates to the variations and the combinations of diseases in hereditary transmission. Besides, both in the choice of its subject and in the whole enterprise of giving this lecture, I have looked for an opportunity of promoting pathology by promoting pathological museums; a motive which, I am sure, will be pardoned, though I am conscious of its being in some measure personal, for I have spent so much time and thought in museums that I feel as if in their greater utility I should myself become more useful.

Now, first, respecting rare diseases, there may seem no want of opportunity of studying them. Our journals and the proceedings of our societies are full of the records of rare cases; many collections of such cases have been published,¹ and there are many rare specimens in every museum. All these have that kind of attraction which belongs to everything that excites our wonder; but we too seldom let the wonder have its proper consequences; we too seldom let it provoke our curiosity so far as to make us search for the meaning and reason of the rarity. There is a question which we should often ask ourselves: Why is any disease rare? at least, why is any rare which does not depend on some accident or some rarely concurring external causes? I shall try to suggest answers which may be, in some instances, sufficient; but I fear

¹ Two of the best are Fournier, art. 'Cas rares' in the *Dict. des Sciences Médicales* (t. iv. 1813), and Eve, *Remarkable Cases in Surgery* (8vo, Philadelphia, 1857). Fournier says that professorships for Lectures on rare cases had lately been established, and Dr. Norman Moore has shown me that Mr. Cross, in his *Sketches of the Medical Schools of Paris* (8vo. 1815, p. 4), enumerates among the regular courses of winter-lectures at the *École de Santé* those on the *Doctrine of Hippocrates and History of Rare Cases*.

that, in more instances, if I can be useful at all, it can only be by suggesting how answers may be found.

First, there is a difference, though it may often seem only a verbal one, between rare cases and rare diseases. A case may be called rare when, though it is evidently one of a common disease, it differs from the usual type or standard of that disease in some one or two features. Thus it is a rare case when a common disease is found in an unusual place : as an epithelial cancer on the upper lip, or this fatty tumour on a finger ;¹ or in unusual quantity, as in this large cartilaginous tumour on a femur,² or in this most rare collection of more than two hundred little nodular masses of loose cartilage which filled a knee-joint and were, this afternoon, removed from it by Mr. Thomas Smith. Again, a case may be rare in respect of the time of its occurrence. For instance, I have lately seen cancer of the rectum ending fatally in a lad of eighteen, and scrofulous abscess in a man of eighty ; and many of us must have seen instances, though they are rare (and these are very important in the history of diseases), in which manifestations of syphilitic inheritance, usually evident in infancy, have not appeared till the time of youth or even of adult age ; or cases may be very rare in respect of accidental complications or of the absence of some usual symptom.

But of all these and other rare cases, the number and variety are so great that it would be impossible to deal generally with them, except as in mere story-telling. It would be very useful if some one would collect hundreds or thousands of them and arrange them, even though it were only under such headings as I have just indicated. But even as they are, singly and in disorder, we ought not to set them aside with idle thoughts or idle words about 'curiosities' or 'chances.' Not one of them is

¹ No. 319. Catal., 1882.

² No. 336. Catal., 1882.

without a meaning; not one but might be the beginning of excellent knowledge, if only we could answer the question, Why is this rare? or, being rare, why did it in this instance happen?

But, because of their number and variety, I must pass by rare cases and will speak only of some rare diseases—that is, of some diseases which are rarely seen and yet occur in a sufficient number of cases, and with sufficient uniformity, and sufficient difference from other diseases, to permit of their being described in general terms, and to justify their being called by distinctive names. And of these again, for they are numerous and various, I shall select only that group which seems most attractive—the group of those, namely, of which there seems reason enough for believing, first, that they were, lately, new diseases and have become more frequent; and, secondly, that they are due mainly to morbid conditions changing and combining in transmission from parents to offspring. I say, due mainly. It is certain that changes in the external conditions of our life have influence on even those morbid conditions which are most personal; but this influence is very hard or impossible to trace in the cases which I have in mind, and it may to-day be neglected though not forgotten. For, in all these cases, the personal factors and those of which alone I have to speak are more potent than the conditional; the inner than the outer. We call these diseases constitutional, diathetic, or by similar names; but the chief fact in them is that they, or the necessary previous states or predispositions to them, are inborn and inbred.

Let me first show that there is reason enough for believing that some rare diseases of this kind were recently—say within the last century—new;¹ and that, more

¹ It may be well to say 'new or renewed'; for it is quite possible that a

recently, though still rare, they have become more frequent. There is, I know, a general unwillingness among pathologists to admit that there are new diseases of this kind; and this unwillingness is often just, for many diseases that may seem new have probably existed long and been overlooked; they may be new to knowledge, but not new in fact. Bright's disease and Addison's disease were new in the sense of having first been well observed and described by those whose names they bear; but no one would venture to say, of diseases so difficult to detect as these used to be, that they did not exist long before they were well observed. We could as well believe that embolism never occurred until just before it was found-out, or that right-side hemiplegia used not to be associated with aphasia. These things were old before they seemed to be new; but how long they had existed neither records nor museums can tell.

It would be, indeed, very interesting if we could tell the time and manner of first appearing in the case of many diseases which are now common; but it is scarcely ever possible. And yet, if you will allow me a digression, let me show what in some instances museums may supply, and what I hope they will in the future supply much more largely. Here are specimens of typhoid ulcers of the intestines preserved by Hunter.¹ Few things have been more important in the knowledge of fevers than the clear proofs of the distinction between typhus and typhoid given by Sir William Jenner in 1850. It was one of the best life-saving discoveries of this century; before it, both disease, even though not chiefly dependent on external causes, may in long courses of years appear, disappear, and reappear.

¹ Nos. 1214-5-7-8, and 1242 (Catal. 1848). Two of them, 1217 and 1242, are represented in Baillie's *Morbid Anatomy*, Fasc. iv. p. ii. figs. 3, 4. The College Catalogue (Ed. 1848, vol. iii. pp. 90, 91) contains interesting observations on the intestinal glands from Hunter's MSS. Other two of the specimens represented by Baillie are in Dr. William Hunter's Museum.

diseases were at least partially misunderstood, and neither was so well treated as now. Since the distinction between them was discovered, it has been possible to trace, in old recorded cases, probable instances of both ;¹ but there is nowhere so clear evidence of the occurrence of typhoid, a century or more ago, as is given in these specimens of Hunter's, preserved without name or history ; not unobserved, and yet not in any fair sense understood. Now, in this, as in many things, Hunter set us a good example. He did not think those things unimportant which he did not understand. He was a thorough naturalist and kept specimens of everything in his field of study which might become useful.

But, however much of what seems to be new we may justly ascribe to our previous oversight of what was old, there yet seems to be evidence enough that new diseases are in progress of evolution and that, as I have said, some of the rare diseases of which I have to speak are the earliest instances of the new.

Good evidence of this kind is to be found, I believe, in the peculiar joint-disease discovered by M. Charcot in association with locomotor ataxy, and in the disease of bones to which I have given the name of osteitis deformans. Neither of these, I believe, was described till within the last few years. They may have been overlooked, but, to believe this, we must believe what is very

¹ See, especially, Hirsch, *Handb. der historisch-geographischen Pathologie*, B. i. p. 158. Dr. Norman Moore has shown a great probability that Prince Henry, son of James I., died of typhoid fever (*Illness and Death of Henry, Prince of Wales*, 8vo, London, 1882). Dr. Wilks has given in the *Guy's Hospital Reports* for 1877 an interesting account of observations anterior to the discovery of Bright's, Addison's, and Hodgkin's diseases. In all these cases, the rule may be applied that they alone should be reckoned as discoverers from whose work may be traced, not merely what might have been the beginning of a discovery, but the continuous history of events consequent on the evidence of its truth.

improbable. We must believe that all the most acute and observant practitioners before our time overlooked, not merely obscure and transient diseases, difficult to study, but cases which lasted many years, and gave constant great distress, and were manifested in signs so plain that they could be recognised in the shape and gait, in the posture and whole aspect of the patients, in strangely large heads and curved limbs. And, further, we must believe that the morbid anatomists before ourselves overlooked changes of structures of the largest, most obvious, and most striking kind. It is, surely, very unlikely that they who studied and recorded such cases as those of extreme rickets and mollities ossium, and even called it *rachitis adulatorum* and kept specimens of it, should have left unnoticed the cases of these two equally and somewhat similarly disfiguring and damaging diseases. This great improbability is strengthened by that which I believe to be a fact—that we have none but recently collected specimens of either of these diseases in our museums; not even among the crowds of bones and joints collected by our predecessors.

In twenty-six years I have seen thirteen well-marked cases of the *osteitis deformans*, and about as many in which it was only partially evident. In the last six of these years I have seen seven of these cases, and others have been published, and yet I cannot find evidence that the disease was ever seen by any of those who had practice like my own. Brodie and Stanley, who saw as many cases of diseased bones as any surgeons of the last generation, had seen no case but that which I showed them more than twenty-five years ago in the patient from whom these specimens were taken. Moreover, I cannot find an old specimen in our museums, or a representation of one in any book of plates, or a description of one in any catalogue.

This might not seem very strange in the case of specimens troublesome or expensive to keep, or in such as are said to 'show nothing.' But these are very striking deviations from health, very plainly to be seen, and dry bones are neither costly nor troublesome to keep. We have large numbers of them collected by Hunter, Howship, Langstaff, Liston, Cooper, Stanley and others, who collected, not merely illustrations of diseases well known to them, but whatever was curious, whether it were understood or not. They would have looked on these bones as gems.

I might repeat this statement in nearly every particular concerning M. Charcot's disease. I believe there is not an old specimen in our museums. There is not one in the Musée Dupuytren; I cannot find a notice or an illustration of one.¹ And yet this disease is now so far from being very rare that Dr. Buzzard has had nine cases under his eye at one time, and several have in recent years been shown in our societies.

Let me adduce one more instance of what I believe to have been new diseases within this century, though the museum-evidence is not so strong as in those of which I have been speaking. Many believe, and, I think, quite rightly, that instances of typical gout, such as gained for it the name 'podagra,' have lately become comparatively rare, and that a large number of less acute diseases, regarded as forms of incomplete or suppressed gout, are much more frequent. It may be that some or many of these lesser forms were always as common as they are now, but were overlooked or were not distinguished from

¹ Mr. Eve (*St. Bartholomew's Hospital Reports*, vol. xvii. 1881) points out a case by Mr. Stanley, in the *Medico-Chirurgical Transactions*, vol. xxiv. p. 122, 1841, which was certainly one of locomotor ataxy and in which both femora were dislocated. But there does not appear to have been any marked change in the head of either bone before its displacement.

other similar ailments. But here are specimens¹ of the effects of phlebitis of the femoral and external iliac veins, which, with the history, may tell that gouty inflammation of the veins was, fifty years ago, if not a new disease, yet a much rarer one than it is now.

Sir Henry Halford saw as much of gout, I suppose, as any man that ever lived; for he was for many years, during a very luxurious period, in the largest practice among the richest people in this town. He gave an account in 1832 of what he called phlegmasia dolens in the male.² The disease so called was common and well known in women after parturition; so that, to justify his essay as a record of cases hitherto unobserved, it was enough for him to speak of phlegmasia dolens as occurring in men. He speaks of it as having been not long before regarded as 'immediately occasioned by a deposit of milk'; but that 'being tested by a more exact pathology,' it was now attributed to 'an inflammation of the veins of the pelvis.' And he says, he was much mistaken if he had not seen three instances of it in men 'within the last few years.' He then relates the case of the nobleman from whom, several years afterwards, this specimen was taken by Sir Astley Cooper—an admirable example of phlebitis, which we may be nearly sure was gouty, showing the changes in the blood-clots and in the walls of the veins during many years.

At the present time, phlebitis of this kind in a male can scarcely be called a very rare disease. There are few, I imagine, in large practice who have not seen many more than three cases within the last few years. So we may believe, I think, that the disease has become more

¹ 1732-3. Catal., 1848.

² Published, in abstract, in the *Medical Gazette*, vol. x. p. 172, May 5, 1832.

frequent in the last fifty years ; and may suspect that not long before Sir Henry Halford's time it may have been a really new disease. It is hard to believe that it could have been overlooked. Its characters are strongly marked and evident to both eye and touch ; it is a very painful, disabling, long-enduring disease, often recurring, sometimes observed in several members of the same family, and commonly leaving the affected limb large, heavy, and clumsy for many years. Could this have been overlooked when similar limbs, in consequence of an allied, though not the same, disease, were known and described in women and while, as it happened, the subject of phlebitis, in its traumatic and pyæmial forms, was being very carefully studied ? For there was a form of phlebitis much more common in the last century than in this—the phlebitis that occurred after bleeding. Hunter had studied this very carefully, had written on it, and shown it in these specimens ;¹ and after his time, both this and the phlebitis after amputation were well known. Especially after the beginning of this century phlebitis following amputation was thoroughly worked-at. It was only three years before the publication of Sir Henry Halford's essay that Mr. Arnott's renowned paper on Inflammation of the Veins was published in the ' *Medico-Chirurgical Transactions.*'² In the same volume are the chief papers by Dr. Robert Lee on phlegmasia dolens ; and he describes cases of phlebitis associated with pelvic cancer ; but not one spontaneous phlebitis is mentioned by either of them.

Now, I think that in all these facts there is enough, not, indeed, to prove, but to justify the belief, that we have here examples of diseases, which have appeared in this

¹ 1722 to 1728. His observations on phlebitis and a case from his MSS. are printed in the Catalogue, 1848, vol. iii.

² Vol. xv. p. 1.

country for the first time within the last century, and which have since become sufficiently frequent, and acquired sufficiently constant and distinctive characters, to be described in general terms and called by new names. Let me repeat: these are not diseases hard to be discerned; they are so well marked, so distressing, so long-enduring, and both during life and after death so large and distinct in all their characters, that it seems impossible that, unless they were very much rarer than they are now, they could have been overlooked.

I think it probable that there are other examples of the kind; but I do not know them, and would rather go on to the second part of my subject: namely, to show the probability, or at least to justify the hypothesis, that these diseases are among the instances of the results of morbid conditions changing and combining in transmission from parents to offspring.

It should hardly be necessary to argue that changes of type in inherited diseases—changes which may be compared with the variations of species or of varieties in natural history—do take place. Yet I venture to think that many of us are prone to think too little of these variations; to regard them as rather unmeaning exceptions, or as the results of some unusual external conditions diverting diseases from their customary course.

It will be better for us if we study, in pathology as in natural history, varieties as much as species; changes as well as the more stable forms. Types of disease there are, standard forms, and the tenacity with which they are maintained—some even from pre-historic times¹—in all

¹ I am indebted to Mr. H. W. Jackson for many references to illustrations of this very interesting subject. The chief of them are in the *Bulletins de la Société de l'Anthropologie*; and an account of them all was given by him in a paper lately read at the West Kent Medico-Chirurgical Society. Mr. Wherry, in the *British Medical Journal*, November 1881, describes bones dug up at Giron with osteo-arthritis well marked.

the varieties of the conditions of our lives is one of the most remarkable facts in all pathology. But they are neither unalterable, nor equally maintained in every individual of a species or a family. Types vary in diseases, as in species; even in the diseases which depend least upon external conditions, and most on the qualities which are transmitted by inheritance.

Let me give some reasons why this must be.

1st. An exact likeness is never transmitted by inheritance: neither an exact likeness of either parent nor an exact composite of both. This is evident enough in features, size, weight, and all that we can observe in external things. If we could be exactly endoscopic we should observe equal variation within; the same want of exact likeness in liver and lung and, I venture to say, in blood and lymph and plasma, and whatever goes to make up the whole person, healthy or diseased. The inheritance of likeness in disease or liability to disease is, indeed, clear evidence of the transmission of likeness in the very minutest part of structure and composition. But the likeness is never perfect; it may in different persons deviate this way or that; it may vary towards disease or back again towards the healthy type; but it is never perfect, and in successive generations its degree of unlikeness may in some persons increase to a great width of difference.

2nd. The certainty and probable extent of this variation must seem the greater if we consider the mingling of diatheses and of all dispositions and liabilities to diseases in transmission from and through both parents. Consider the difficulty of maintaining a 'breed' in any of the varieties of the species domesticated or cultivated by us—in horses or dogs, in pigeons, or in seedling plants; the care that both parents should be of the same blood, or

the same race, and that their produce should be raised in all due conditions ; and then consider how numerous and wide, in spite of all this care, are the deflections from the type. With these facts before us we cannot imagine that diseased conditions should often be transmitted singly and unchanged ; it is, surely, not likely that disease should be transmitted with more fixed conformity to type than normal compositions are. Hybrids and mongrels must be even more common among diseases than among species and varieties.

3rd. In studying the variation of diseases by combining or convergence of inherited qualities, we may not limit our thoughts to a single generation. It is reasonable to believe that instances occur of reversion, in which diseases or tendencies to disease may appear after a lapse of many generations. Such, I expect, are some of the cases in which leprosy has been seen, even of late years and in this country, in persons never exposed to any of its external causes ; and to the like of this we may ascribe, I think, some of the rare cases which defy all efforts to refer them to any combination of types of disease now prevalent.

4th. In all thinking about the transmission of diseases, we must have in mind how greatly our own race and the creatures that we take care of differ from all others in respect of the maintenance of life in varied forms and under changed conditions. Few varieties of our domestic animals or of our cultivated plants would exist long in the wild state, unless they could quickly revert from the forms which they have acquired in their artificial existence. Certainly, in an open struggle for existence, Brown-Séquard's epileptic guinea-pigs and all such altered creatures would have a poor chance of propagating their likes. Yet with care even these live and multiply. Much more, among ourselves and other cultivated races, the constant

changes in those things among which life is spent, and those by which it is maintained, must add to the number, the diversity and the extent of variations from the type of health. But, through the benevolence towards the weak which is a constant part of educated humanity, there are few deviations from the type of health which are not maintained and transmitted, whether alone or in combination.

Now, I half wish that I could escape from the necessity of testing my doctrine by my facts ; but, as I have often asked myself, so others may ask—How can the cases of rare diseases of which I have been speaking be explained as the results of morbid conditions changing and combining in transmission from parents to offspring ?

In the phlebitis we may often trace a variation from the customary type or standard of the very old and heritable disease, gout. In many cases its relations to typical gout are clear. The patients are members of gouty families, and in many of them other signs of gout are evident, either coincidently with the phlebitis or at other times ; it has, in short, ample evidences of being one of the many forms of what is called ‘incomplete gout.’ But, for a reason why this variety of gout settles (if I may so speak) in veins, especially in those of the lower extremities, I can only guess at a convergence of inherited dispositions both to a modified form of gout and to some conditions of veins rendering them, among all the structures, the most sensitive to the gouty process. Certainly it is not accident which determines the disease to the veins, for this disease, as the saying is, ‘runs in families.’ I know of its occurrence in two brothers and three of their cousins ; and I have heard Sir Charles Locock tell of four sisters who had phlegmasia dolens, and whose father had crural phlebitis.

I am conscious that this is little more than guessing,

and for the osteitis I must guess still further; or, rather, let me say that, to the furthest bounds of propriety, I must exercise that use of the imagination which may happily discern a way towards the truth. I imagine, then, that a likeness of the osteitis deformans to several other diseases may indicate a combination, in definite proportions, of transmitted dispositions to those diseases; a combination which has become possible by changes of the type of one or more of them. First, it shows some relationship to mollities ossium and rickets, for, though it is an inflammatory disease, which they are not, yet the softening which permits of the curving of the bones is distinctive, and hardly occurs in any other form of inflammation of bone in middle or later life. And, again, the relation of the osteitis to rickets and mollities ossium is notably indicated in the porous thickening of the skull, which is found in some instances of them all. It is evident in this osteitic skull¹ which is figured in the 'Medico-Chirurgical Transactions';² and in this portion of the skull described in Mr. Solly's celebrated case of mollities;³ and it is well marked in these specimens of genuine rickets from erroneous diet in young lions and young monkeys.⁴ Further, there appears some relation to gout, for some of the cases have a known inheritance of gout, and instances are sometimes seen, in typically gouty persons, of a single bone having all the characters of the osteitis, though all the other bones appear healthy. Such an one is this femur, for the opportunity of showing which I am indebted to Mr. Bowlby, of St. Bartholomew's. There is a likeness also, it may be said, to the osteo-arthritis and other forms of rheumatic gout in the remarkable maintenance of good general health during even

¹ No. 1239. Catal., 1882.

³ No. 733.

² Vol. lx. pl. 5.

⁴ No. 711, &c.

many years of a painful and crippling inflammatory disease; and, further, there appears some relationship to cancer in the singular frequency with which cancer or sarcoma occurs in the healthy bones or other parts of those who have suffered for many previous years with osteitis deformans.

Thus, I imagine, by inherited dispositions, accumulating and combining or converging in definite proportions, this disease may be produced. I would try to imagine the genealogy of M. Charcot's disease, but that I have too little clinical knowledge of it. I can only suggest a combination of osteo-arthritis with syphilis chiefly localised in some spinal nerve-centre; but I believe far better suggestions may be made by those who, suspecting a combination of diseases rather than many radiating from one source, will carefully study the essays of Professor Charcot and Dr. Buzzard's admirable 'Clinical Lectures on Diseases of the Nervous System.'

Besides, I may seem to have guessed already more than enough. And yet, even if my guesses are wrong, my error of interpretation does not weaken the probability of the belief that these and other rare diseases of like kind are instances of settled varieties of diseases, severally due to variation and convergence of morbid conditions in hereditary transmission. And if this be in any measure true, or even not more than a reasonable hypothesis, then it must be of great importance that we should know much more than we yet do of the variations which, in progress of time, diseases, or certain examples of them, may undergo; of their deviations in a gradually increasing number of instances from typical or standard forms; their acquirement in those instances of other comparatively fixed and long-abiding characters; of the occasional disappearance of old forms of disease, and the evolution of

new ones. Moreover, we should strive to learn whether in their convergence they are as hybrids, rarely propagating their likes, or as mongrels, propagating with freedom and with little change ; whether they co-exist without combining, or combine like new chemical compounds.

All these variations in diseases should be studied as Darwin studied the variations of species. Let me be clear in saying, as Darwin studied ; for in the pursuit of new knowledge he may be a model to all, as he has been to me so far as I could imitate him. He, I know, would have studied these things, not by deduction, as from a law exactly formulated and from which he could trace the course of every change, but by a most careful collection of facts ; facts to be seen in specimens and read in full records and stored in museums, and by a study as complete for every case as if no law of evolution had ever been discovered.

The study of these variations of diseases is not one of mere pathological curiosities. It may be of great practical utility ; let me show how, if only that I may provoke some to pursue it vigorously to whom mere pathology is not attractive.

We hear much, and often, of the uncertainty of medicines ; of disappointments in the use of this or that supposed remedy ; and substances which have long been in good repute for the treatment of this or that disease are spoken-of with disrespect. It need not be questioned that in many cases the belief in the utility of a medicine has been maintained by completely erroneous observations. Such was the belief in the utility of infinitesimally small doses of anything ever yet swallowed. And other beliefs less evidently absurd may have been nearly as ill-founded. But there are many of which this is not to be said. It cannot be doubted that bromide of potassium is often very

useful in epilepsy; yet sometimes, as we say, it fails; or that guaiacum is useful in some cases of chronic rheumatic arthritis, and is in others very disappointing; or that arsenic sometimes does and sometimes does not do good in cases of lymphadenoma. I suppose there is not a medicine in the pharmacopœia which does not sometimes disappoint him who gives it hopefully; not one which is not, therefore, spoken of with contempt or blame, as if it were a responsible agent convicted of default. But here is an unfair imputation. It is not these medicines which are in fault, but ourselves. That which some call the fallacy of therapeutics is generally the fallacy of diagnosis. To state the facts roughly, we suppose cases to be alike which are really different; and, very naturally, the medicine that does good in some of them is useless in others. For example, in the group of cases which I chiefly have in view, we do not always discern when a disease has varied so far from its usual type that it is no longer amenable to its usual remedies. A better diagnosis must precede a better therapeutics. We need not only the diagnosis between diseases essentially different, but that between the different and varying forms of each of those which we call by a generic name; and beyond this, we need a more exact power of what may be called analytic diagnosis; for there are few simple cases, and in those which are not simple we need to be able to discern all the components, and the proportions in which they are mingled or combined. Better treatment will follow better diagnosis, and better diagnosis will certainly follow a more exact pathology.

I may illustrate this with an instance which is, besides, of some interest in the study of the variations of transmissible diseases and of the utility of museums. Questions are often asked as to changes which syphilis may, in course

of time, have undergone; and especially, whether the internal organs were always, as they are now, liable to its attacks. It is hard to answer such questions on the evidence of any existing records; indeed, I may cite the whole history of syphilis as an instance of the inefficiency of records for the tracing of the natural history of diseases. But here is something suggesting what museums may do: a portion of muscle preserved by Hunter,¹ and at least a century old, in which are morbid changes which may be safely referred to syphilitic gumma. Probably, similar evidence may be found in other museums; and there are other facts significant of the existence long ago of these internal syphilitic diseases, as well as of the improved treatment following better diagnosis. Fifty years ago it was the custom, as it long had been, to give mercury, not only in all recognised syphilitic cases and in most acute inflammations, but in a large number of cases of which one could scarcely say more than that they were all chronic and all obscure. Especially there were many such cases of what were considered chronic inflammation of the eyes, and of the brain and spinal marrow, the liver, and the testicle. To all of these cases it was customary to give mercury till, as one said, 'the mouth was touched,' and thus some were cured, and some uncured, and some harmed. The cures were enough to keep the mercury in such good repute that it was given more and more generally; and then the disappointments, as they were called, became too many, and the mercury was blamed, and was almost disused for chronic inflammations.

But, meantime, a more exact pathology, a pathology more exact both in its morbid anatomy and in its clinical studies, was discovering the previously unsuspected

¹ No. 583. Dr. Wilks, *l. c.*, mentions specimens of syphilitic lardaceous liver at least fifty years old in Guy's Hospital Museum.

syphilitic diseases of internal organs, and with this better pathology there came a better diagnosis, and with the better diagnosis a more judicious use of mercury, and good reason to believe that the chronic and obscure cases which mercury used to cure were those of syphilis overlooked. The case is an exemplary one of the relations between the true pathology and the right treatment of disease, exemplary not only for encouragement, but for method of study ; for the study was both clinical and anatomical, in the living and in the dead, with records and with specimens. Such must be our study of all the cases which I have chosen to speak of—the cases in which diseases deviate from their usual type, or combine in various proportions. But there are some rules in study which are especially applicable to these cases.

1. We should very carefully study all cases which are not according to an admitted type. We should study all exceptions to rules ; never thinking of them as unmeaning or accidental. Especially, we should never use, in its popular, but wrong, translation, the expression ‘*exceptio probat regulam*’ ; as if an exception to a rule could be evidence that the rule is right. If we use it, let this be with a better meaning ; translating it, as surgeons should, that an exception probes a rule ; tests it, searches it to its very boundary : as the Bible says we should ‘*prove all things.*’ In this meaning, the words may be an excellent motto for the study of all diseases that deviate from types.

2. We should look-out for indications of the existence in the same person of two or more morbid conditions or dispositions such as may be derived from both parents or from several ancestors. For, as in plants and animals there are hybrids and mongrels, or, as in chemistry, many compounds and mixtures, so are there in diseases. We

see them in the multiform varieties of what we have to call rheumatic gout; in gout crossed with scrofula; in syphilis crossed or mingled with scrofula or with gout. It is often not difficult to analyse some of these combinations among our cases, especially if we study those lesser signs by a concourse of which, only rather less surely than by their standard signs, each of these diseases may be discerned; and I know few things in practice more useful than to be able, even in some instances, to adjust our treatment to the proportion of each disease in the compound. But we may be sure that there is much more to be learned in this direction; and it is best to believe that we rarely have to do with a simple and unmixed morbid constitution. There are few worse habits in practice than that of commonly saying of one case 'It is all gout,' and of another that it is all scrofula, or all syphilis. We might as well say of any Englishman that he is all Norman, or all Anglo-Saxon, or all Celt. We may, indeed, sometimes see persons who appear to be as types of races unchanged in many centuries, but in practice we had better study every man as, for better for worse, a composite of many ancestors.

3. We should have for all these cases a much more complete and exact study of all the personal conditions of disease than is now usual. Of course this should include all that can be learned of each patient's family history; though there are few parts of medical inquiry more fallacious than this often is; and at the best it will need, besides, the exactest study of the patient's self. Perhaps the brilliant success which has been achieved by the recent studies of disease-producing organisms or other materials acting on us from without.—a success not equalled in any other field of medical inquiry.—has made some think too little of those changes within ourselves.

which occur in such ordinary conditions of life that they may be called spontaneous. Yet these are not less important in the production of diseases, and these must be studied; just as in agriculture soils must be studied as well as seeds. This is true even in respect of those diseases whose essential causes are most evidently external, even of those which are due to specific contagia: their germs or seeds, if I may so speak, will not germinate in an unfit soil. I suppose there is not a day in which most of us do not inhale or come in contact with the germs of some frequent or contagious disease; but they do not germinate in us any more than do the seeds of tropical flowers in our streets or in the fields to which the wind scatters them; we do not offer the fitting soil. And even among those in whom they do germinate, the product varies according to the soil. And the study of this soil, this living soil, is yet more necessary in respect of diseases which come, in part or wholly, by inheritance; for it is in each as personal and distinct as any other constituent of personal character, and the study of it must be intimately personal, with an exact analysis of every disposition to disease. The aim of pathologists in this direction should be for knowledge like that of the keen family-practitioner, who, as he says, knows the constitution of every member of a family.

All this is equivalent to saying that these variations in diseases must be studied both in practice and in scientific pathology. It is hopeless that either a practitioner who thinks lightly of pathology, or a pathologist who thinks lightly of observant practice, should do more in the study of these questions than attain to that measure of partial truth which is often as deceptive as error. Each must be tested by the other. The living and the dead must be alike and equally studied; and the dead must be studied

in exact observations, with accurate records and, especially, with museums.

I need not dwell on the value of good records, good descriptions, and good photographs or other representations of diseases; but they never have been and, probably, never will be, enough. For we can record only what we see and observe; and even the best of us are apt to observe only such things as are in close relation with what we believe that we already know. Moreover, many of the things which we do observe we do not record, because they seem useless or because they cannot be incorporated with other knowledge; and in the things which we record we often state only the most obvious facts. For these reasons—to say nothing of the things which we altogether overlook—our records, whether in writing or in portraiture, are insufficient, and we need to have, with them, museums in which changes of structure may be preserved for repeated and revising study and comparison. For instance, in regard to the group of diseases of which I have been speaking, we ought to have in our museums specimens in which we might study all the gradations of change of structure from type to type, all the changes due to mingling of forms, all varieties of diseases, all hybrid forms. We need to be able to study all these things, as the naturalist or the comparative anatomist needs his specimens; not only for the teaching what is already known, but for continued re-examination and continued additions to his own knowledge.

And for complete study we must have large museums showing the large naked-eye characters of diseased structures. I am sure no one will think me likely to depreciate the microscope; it has added, and will continue to add, more than can be told to our knowledge; but it has not diminished the absolute value of other evidence; and in

pathological anatomy, as in all our sciences, there are many instances in which the naked eye sees facts with more meaning than the microscopic one can.

This is especially true in the case of morbid structures resulting from nearly allied diseases and, therefore, especially true for those of which I wish to urge the study. In morbid structures, as in species, the nearer the alliance the less are the differences to be found in minute structures, and the more must we depend for distinctions on the study of visible shapes and sizes and constructions. I suppose that we could not with the microscope distinguish the human skeleton from that of the monkey; certainly, we could not distinguish one human skull from another in all those varieties of national form which are collected in our museum. And so it is in many instances of morbid bone-formation. I doubt whether microscopic examination could detect characteristic differences in each of this group of specimens. But with the naked eye it is sure that this is a syphilitic node on a tibia, and this a growth beneath a chronic ulcer over the shin, and this a pedicled exostosis or ossified cartilaginous outgrowth from the shaft of a long bone, and this an instance of osteo-arthritis, and this a portion of the skeleton of an osteo-sarcoma or osteoid cancer.

Moreover, it is to be observed that in morbid structures, as in those that are natural, in the same proportion as the aggregated elements of embryonic structures acquire their complete and final form, so do the bodies composed of them acquire distinctive shapes and methods of construction plain to the unaided senses. The ova of many species may seem alike both in outer shape and in their component elemental structures. But in proportion as these structures are differentiated, and developed into their higher and abiding forms, as into nerve-fibre, and

muscular-fibre, and the rest, so the larger characters of even the nearly allied species—the characters of shape and size and appropriate construction of the whole body, and of each part of it—become more and more different ; and these constitute the really distinctive characters of each species.

And so it is in morbid products. The acquirement of distinctive shapes and methods of construction coincides with the development of elemental forms. For example, in these sarcomata are only the lowest elemental structures, round cells, spindle cells, and shapeless plasma ; and the masses thus combined are shapeless, featureless, decisive by negation. But in these fatty and fibrous and cartilaginous and bony tumours, in which the elemental structures have advanced to higher forms, the masses which they severally compose are almost as characteristic and distinct in visible shape and construction as are the several normal organs of the body.

In every case, then, both the largest and the smallest characters should be studied. The naked eye can discern one set of facts, the aided eye another ; both are essential to complete knowledge ; no one should be content with either, for neither is alone sufficient. So we must have large specimens as well as small ones, and certainly large ones for the study of the gradual variations of diseases as they deviate from typical forms and become variously mingled.

And now, as I come near to my term of time, let me, as is customary in certain other places, conclude with an earnest appeal to your liberality. We want liberal contributions, not of money, but of specimens to our museum. We want specimens of many kinds ; of course we want whatever is rare, but not these alone ; we want some to complete our series of typical specimens ; and (to keep to

the chief subject of my lecture) we want the opportunity of choosing among many of what are called 'bad specimens.' We are all ready to collect what are called good specimens as being well-marked instances of the standard characters of diseases, and to put aside as 'bad' those which deviate from those characters, just as, clinically, we speak of good and bad cases of a disease. Of course, good specimens, typical specimens, must be at hand for the teaching of pupils who have to study illustrations of the accepted descriptions of diseases; but it is among bad specimens, even as it may be among exceptional cases, that those who are past pupilage, though they have not ceased to be students, may study the variations of disease.

I ask the more boldly for contributions to the pathological collection because of its present satisfactory condition and the activity of work in it. You will soon see it in the repaired and renovated building. Looking at the number and value of the specimens and the wide range of pathology which they illustrate; looking at the interest of the history of our science which is told in many of them; at the memorials of Hunter and Matthew Baillie, of Astley Cooper, Liston, Howship, Lawrence, Hammick, Fergusson, Hilton and many more; looking forward to what the museum will tell of the researches and skill of those who are still with us, and among whose names I venture to feel sure, Mr. President, that none will take precedence of your own, while men study the specimens with which your skill and just audacity in operating have enriched the series of diseases of the ovaries and uterus: looking at all these things, and then at the perfect order and condition in which the specimens are preserved, I feel that the collection is one which all we members of the College may feel personal pride in calling our own, and should feel a personal duty to enrich.

And its utility is being constantly more appreciated. I have often been made happy by the contrast which I have seen while working at the new edition of the catalogue. While I was writing the last edition, between thirty and forty years ago, scarcely a student ever entered the museum. Hour after hour I sat alone; I seemed to be working for no one but myself, or for nothing but the general propriety that a museum ought to have a catalogue, though no one might ever care to study with it. Now, and for some years past, a day rarely passes without many pupils and others being at work in every part of the museum.

All this is good; but much more is to be done. Our museum should be, even more than it is, the centre in which all pathologists may find help in searches after that which is not yet known; in such searches, for example, as may lead to a complete knowledge of the variations of diseases. For many years, even from the beginning, the anatomical and physiological departments of our museum have been not only a noble collection of specimens, but, through the renown and learning of its conservators, a great centre of teaching. Scientific men, especially comparative anatomists and anthropologists, have known that here, if anywhere, they could find whatever help a museum and a master in those sciences could give. A fortnight ago the President of the Royal Society, presenting one of the Royal Medals to Professor Flower, said: 'Professor Flower has been for more than twenty years conservator of the museum of the Royal College of Surgeons; and it is very largely due to his incessant and well-directed labours that the museum at present contains the most complete, the best ordered, and the most accessible collection of materials for the study of vertebrate structures extant.'

It is not for me to praise the pathological collection with similar words. But great as may have been its utility hitherto, we may be confident that it will henceforth be more useful than ever. In the vast increase of the biological sciences it became impossible that one man should be nearly complete in the knowledge of both natural and pathological anatomy.¹ I say impossible. I believe there is not such an one living; if there could have been one it might have been Mr. Flower. Now, we may hope that labours as 'incessant and well-directed' as his will be devoted especially to the pathological collection.

It is known to many of you that Sir Erasmus Wilson, in his usual liberality, gave the College 5,000*l.*, of which the interest should be spent in the promotion of pathology; and he agreed that this would best be done by helping to the appointment of a Curator of the pathological department of the museum; and we have an admirable one. Mr. Eve is a worthy colleague and helper of Mr. Flower, excellent, like him, not only in knowledge, but in that which is even more rare, the love of museums, and of all that belongs to their maintenance and illustration, even to the making of catalogues. In all these good qualities he has distinguished himself at St. Bartholomew's. I believe that we may rely on him for making so good use of the museum, and of all that can be brought to it, that the College shall be the chief centre for the

¹ In 1871, Mr. Flower, in his annual Report, wrote: 'Pathology cannot be successfully cultivated by anyone who has to devote his whole time to the business of the Institution, without opportunity of ever seeing living patients or recent post-mortem examinations.' The office of Pathological Assistant was then instituted; and it was well filled, for six years by Dr. Goodhart and for four years by Mr. Doran. 'The good result of this experiment,' as Mr. Flower reported in 1881, 'led to the consideration as to whether it could not be extended'; and the consideration led to the institution of the Pathological Curatorship.

study of pathology, even to the furthest point at which it can be studied in specimens of diseased structure. I beg your help that it may be so ; and if I shall have helped to-day to this good result, the first Bradshawe lecture in our College will have well fulfilled the purpose of its founder.

XIX.

NATIONAL HEALTH AND NATIONAL WORK.

An Address given at the Albert Hall, June 18, 1884, at the Reception, by H.R.H. the Prince of Wales, of the International Juries of the Health Exhibition. Sir James Paget was a Vice-President of the Exhibition.

MAY IT PLEASE YOUR ROYAL HIGHNESSES, YOUR EXCELLENCIES, MY LORDS, LADIES, AND GENTLEMEN,—It was very difficult to select, from the vast number of subjects relating to Health and to Education, one on which I could fitly speak to-day. On general education I could not venture to speak; and, believing that I should have to address a large and various audience, I thought it would be best to choose a subject by which I might urge one of the chief objects of this Exhibition, and one which I know that you, Sir, have always had in view, namely, that the public themselves should consider, much more than they do, the utility and the means of maintaining their own health. I have, therefore, chosen the relation between National Health and Work; especially as it may be shown in a few of the many examples of the quantity of work which is lost to the nation, either through sickness or through deaths occurring before the close of what may fairly be reckoned as the working time of life. I think it may be made clear that this loss is so great that the consideration of it should add largely to the motives by which all people

may be urged to the remedy of whatever unwholesome conditions they may live in. It is a subject which is often in the minds of the real students of the public health, but the public itself is far too little occupied with it.

I shall speak only of National Health ; of health in its relation to national welfare. In consideration of his own self a man may be deemed healthy who lives idly, comfortably and long ; who enjoys every day of his life, and satisfies every natural appetite without consequent distress. And when such an one dies of old age, with a timely, uniform and painless decay of every part, he may be deemed to have been completely healthy. And yet it is possible that he may have enjoyed his own health in the midst of a poor, unhealthy and unhappy nation, to which he has done no good whatever.

If we could find a nation composed of people such as this man, we might be bound to speak of them as healthy ; but we should be right in calling the whole nation utterly unsound, and might safely prophesy its complete stagnation or its quick decline and fall.

It is not health such as this—idle, selfish, unproductive—that we want to promote either in the individual or in the multitude. Comfortable idleness, such as that of some vagrants and some fine gentlemen, is a despicable result of good health : it is what no thorough man would ever wish for. In view of the national health and welfare, the pattern healthy man is one who lives long and vigorously ; who in every part of his life, wherever and whatever it may be, does the largest amount of the best work that he can, and, when he dies, leaves healthy offspring. And we may regard that as the healthiest nation which produces for the longest time and, in proportion to its population, the largest number of such men as this,

and which, in proportion to its natural and accumulated resources, can show the largest amount and greatest variety of good work.

Here let me insert, as an interpretation clause, that in all this and what is to follow the word 'man' means also 'woman,' and 'he' means also 'she'; and that when I speak of work, I mean not only manual or other muscular work, but work of whatever kind that can be regarded as a healthy part of the whole economy of the national life. And I shall take it for granted that a large portion of all national welfare is dependent on the work which the population can constantly be doing; or, if I may so express it, that the greater part of the national wealth is the income from the work which is the outcome from the national health.

It is a common expression that we do not know the value of a thing till we have lost it; and this may be applied to the losses of work which are due to losses of national health. There are very few cases in which these can be estimated with any appearance of accuracy; but I am helped to the best within our present reach by Mr. Sutton, the Actuary to the Registry of Friendly Societies. In his office are the returns, for many years past, of the sickness and mortality among the members of a very large number of these societies; and, among other things, there is recorded the number of days on which each member, when 'off work' on account of sickness, receives money from his society. Hence Mr. Sutton can estimate, and this he has been so good as to do for me, the average number of days' sickness and consequent loss of work among several hundred thousands of the workmen and others who are members of these societies. From the entire mass of these returns, he deduces that the average number of days' sickness, per member, per annum, is very

nearly $1\frac{1}{2}$ weeks; and this agrees, generally, with the estimates made in other societies by Mr. Neison and others. But the averages thus obtained include the cases of members of all ages, and among them many cases of chronic sickness and inability to work during old age. In order, therefore, to get a better idea of the actual annual loss of work through sickness, he has taken the published experience of the members of the large group of friendly societies known as the Manchester Unity of Odd Fellows; and then, on the fair assumption that the rates of sickness of the whole population during the working years of life would not be far different from this experience, he has calculated the following tables, showing the average annual rates of sickness of each person, enumerated in the Census of 1881 as living between the ages of 15 and 65.

Ages	Number of Males: Census of 1881 (England and Wales)	Weeks' Sickness per annum, accord- ing to the ex- perience of the Manchester Unity	Average Sickness per individual per annum (in weeks)
15—20	1,268,269	844,428	·666
20—25	1,112,354	820,183	·737
25—45	3,239,432	3,224,134	·995
45—65	1,755,819	4,803,760	2·736
All ages from 15—65	7,375,874	9,692,505	1·314

Ages	Number of Females: Census of 1881	Weeks' Sickness per annum, accord- ing to the ex- perience of the Manchester Unity	Average Sickness per individual per annum (in weeks)
15—20	1,278,963	851,701	·666
20—25	1,215,872	896,685	·737
25—45	3,494,782	3,476,146	·995
45—65	1,951,713	5,368,229	2·751
All ages from 15—65	7,941,330	10,592,761	1·334

Briefly, it appears from these tables that the average time of sickness among the male population during the working year is 1·314 weeks—that is, a small fraction more than 9 days each in each year—and that among the female population it is yet a small fraction more; the excess arising from the larger proportion of females at the older ages. The result is that among males there is a loss of 9,692,505 weeks' work in every year, and among females a loss of 10,592,761 weeks. Thus we may believe that our whole population between 15 and 65 years old do, in each year, 20,000,000 weeks' work less than they might do if it were not for sickness.

The estimate is so large that it must, on first thoughts, seem improbable; but on fair consideration I believe it will not seem so. For the members of the Manchester Unity who are in the working time of life, the reckoning is certainly true, and it is founded on the experience of between 300,000 and 400,000 members. In respect of health they may represent the whole population, at least as well as any group that could be taken. They are not very strictly selected, they are not picked lives, yet they are such as are able, when they are in health, to earn good wages or good salaries, and, as their prudence in joining this association shows, they are comparatively thrifty and careful persons. They do not, at all events, include many habitual drunkards, cripples or utterly invalids, or of those who, through natural feebleness or early disease, or mere profligacy, cannot earn enough to become members or maintain themselves in membership. Neither do they include many of the insane or imbecile and idiotic, of whom there are, in our population, nearly 70,000, doing no work, and losing not less than $3\frac{1}{2}$ millions of weeks' work in the year.

It would be tedious to tell the grounds on which the

estimate may be deemed too high, for just as many and as good could be told on which it might be deemed too low. And it is rather more than confirmed by some estimates of the annual sickness in other and very different groups of persons.

In the Army, at home, the average number of days' sickness in each year is, for each soldier, about 17; and, as the number of the troops in the United Kingdom is more than 80,000, we have here a loss of about 200,000 weeks' service in each year.

In the Navy, on the home stations, the average number of days' sickness in each year has been in the last five years for each man nearly 16; so that for the total of about 20,000 men there is a loss of 45,000 weeks' service in each year.

The amount of sickness in the Services thus appears much higher than in the Friendly Societies. This is due, in great part, to the fact that a soldier or a sailor is often put off duty for a day or two for much less illness than that for which a civilian would 'go on his club.' Still, the one estimate may confirm the other; for the sickness in the Army and Navy is that of picked men, who were selected for the Services as being of sound constitution, and who are in what should be the best working years of life: and if it includes many cases of sickness for only a day or two, it excludes nearly all cases of more than a few months, such as make up a heavy proportion of the average sickness in the Friendly Societies and in the general population.

And I may add that the estimate from these Societies, that 9 days in the year may justly be thought a fair estimate of the working time lost by sickness, is confirmed by the records of sickness among the 10,000 members of the Metropolitan Police Force; for among these, including

cases of long illness such as are also in the Societies, the average is more than 9 days in the year.

I think, then, that we cannot escape from the reasons to believe that we lose in England and Wales, every year, in consequence of sickness, 20,000,000 of weeks' work ; or, say, as much work as 20,000,000 of healthy people would do in a week.

The number is not easily grasped by the mind. It is equal to about one-fortieth part of the work done in the year by the whole population between 15 and 65 years old. Or, try to think of it in money. Rather more than half of it is lost by those whom the Registrar-General names the domestic, the agricultural and the industrial classes. These are rather more than seven millions and a half in number, and they lose about 11,000,000 of weeks ; say, for easy reckoning, at a pound a week ; and here is a loss of 11,000,000*l.* sterling from what should be the annual wealth of the country. For the other classes, who are estimated as losing the other 9,000,000 weeks' work, it would be hard and unfair to make a guess in any known coin ; for these include our great merchants, our judges and lawyers, our clergy and medical men, our statesmen and chief legislators ; they include our poets, and writers of all kinds, musicians, painters and philosophers ; and our Princes, who certainly do more for the wealth and welfare of the country than can be told in money.

Before I speak of any other losses of work or of wealth due to sickness, permit me, as in parenthesis, to point out to you how very imperfectly these losses are told or even suggested by our bills of mortality. These, on which almost alone we have to rely for knowing the national health,—these tell the losses of life ; and more than misery enough they tell of ; but to estimate rightly the misery of sickness and the losses of all but life that are due to

it, we need a far more complete record than these can give.

Take, for example, such a disease as typhoid fever—that which Mr. Huxley has rightly called the scourge and the disgrace of our country. It has of late destroyed, in England and Wales, among persons in the working time of life, nearly 4,000 in the year. Its mortality is about 15 per cent., so that if, in any year, 4,000 die of it, about 23,000 recover from it. Of these, the average length of illness is, on the authority of Dr. Broadbent, about ten weeks. Here, therefore, from one disease alone, and that preventible, we have an annual loss of 230,000 weeks' work, without reckoning what is lost with those who die. And the same may be said of nearly all the diseases that are most prominent in the bills of mortality. The record of deaths, sad as it is, tells but a small part of the losses of happiness and welfare that are due to sickness. It is as if, in a great war, we should have a regular return of the numbers killed, but none of the numbers sick or wounded, though these, more than the killed, may determine the issue of the war.

Let me now tell of another loss of work and of money through sickness and early death. In all the estimates I have yet referred to, no account is taken of those who are ill or die before they are 15 years old. They are not reckoned as in the working-time of life, though in some classes many thousands of them are. [In the domestic, agricultural and industrial classes of the Registrar-General nearly half a million of them are included.] And yet the losses of work due to sickness among children must be very large. Consider the time which might be spent in good productive work, if it were not spent in taking care of them while they are ill. Consider, too, the number of those who, through disease in childhood, are made more

susceptible of disease in later life, or are crippled, or in some way permanently damaged ; such as those who become deaf in scarlet fever, or deformed in scrofula or rickets, or feeble and constantly invalid, so that they are never fit for more than half work, or work which is only half well done. These losses cannot be counted, but they must be large. There are others more nearly within reckoning ; the losses, namely, which are due to the deaths of those who die young. It may justly be said that all that they have cost during their lives is so much money sunk ; so much capital invested and lost. If they had lived to work, their earnings would have been more than sufficient to repay it ; but they have died and their cost is gone without return. The mortality of children under 15 in 1882 was nearly a quarter of a million. What have they cost ? If you say only 8*l.* apiece, there are more than 2,000,000*l.* sterling thus lost every year. But they have cost much more than this, and much more still is lost by the loss of the work they might have lived to do.

It is, indeed, held, I believe, by some that these things should not be counted as losses : that we have a surplus of population, and that really the deaths of children, though they may be the subjects of a sentimental sorrow, cannot reasonably be regretted. I cannot bring myself to admit that such a thing should even be argued. I have lived long in the work of a profession which holds that wherever there is human life it must be preserved ; made happy, if that can be ; but, in any case, if possible, preserved ; and no argument of expediency shall ever make me believe that this is wrong. Indeed, I am rather ashamed—even for the purpose that I have in view—to use so low an argument as that of expediency in favour of the saving of health and of life. I am ashamed of making money appear as a motive for doing

things for which sufficient motives might be found in charity and sympathy and the happiness of using useful knowledge ; but it seems certain that these are not yet enough for all that should be done for the promotion of the national health ; therefore, it seems well to add to them any motives that are not dishonourable ; and so I add this, that we lose largely not only in happiness but in wealth by the deaths of these poor children.

I will add only one more illustration of these losses, which is always suggested by looking at tables of mortality. The deaths of persons between 25 and 45 years old, that is during what may be deemed the 20 best working years of life, are annually between 60,000 and 70,000 ; in 1882 they were 66,000. Think, now, of the work lost by these deaths ; and of how much of it might have been saved by better sanitary provisions. If one looks at the causes of their deaths, it is certain that many might have been prevented, or, at least, deferred. Say that the 66,000 might have lived an average of 2 years more ; and we should have had from them alone, in this year and last, an increase of work equivalent to that of at least 6,000,000 weeks ; as much, in other words, as 6,000,000 people could do in one week.

More instances of losses of work by sickness and premature death might easily be given, but not easily listened to in this huge hall. Let these suffice to show something of our enormous annual loss, not only of personal and domestic happiness—that is past imagining—but of national power and wealth. Surely we ought to strive more against it.

But, some may ask, can these things be prevented ? are they not inevitable consequences of the manner of life in which we choose or are compelled to live ? No ; certainly they are not. No one who lives among the sick

can doubt that a very large proportion of the sickness and the loss of work which he sees might have been prevented ; or can doubt that, in every succeeding generation, a larger proportion still may be averted, if only all men will strive that it may be so.

Let me enumerate some of the chief sources of the waste as they appear to oneself in practice.

Of the infectious fevers, small-pox might be rendered nearly harmless by complete and careful vaccination. Typhus and typhoid, scarlet fever and measles might, with proper guards against infection, be confined within very narrow limits. So, probably, might whooping cough and diphtheria.

Of the special diseases of artisans there are very few of which the causes might not be almost wholly set aside. Of the accidents to which they are especially liable, the greater part, by far, are due to carelessness.

Of the diseases due to bad food and mere filth ; to intemperance ; to immorality ; in so far as these are self-induced, they might, by self-control and virtue, be excluded. Scrofula, rickets, scurvy, and all the wide-spread defects related to them, might be greatly diminished.

It can only be a guess, but I am sure it is not a reckless one, if I say, that of all the losses of work of which I have spoken, of all the millions of weeks sadly spent and sadly wasted, a fourth part might have been saved, and that, henceforth, if people will have it so, a still larger proportion may be saved.

We may become the more sure of what may be done by looking at what has been done already. Let me show some of it ; it will be a relief to see something of the brighter side of this picture.

In a remarkable paper lately read before the Statistical Society, Dr. Longstaff says : ‘ One of the most striking

facts of the day, from the statistician's point of view, is the remarkably low death-rate that has prevailed in this country during the last eight years.' In these years the annual death-rate has been less than in the previous eight years, in the proportion of two deaths to every 1,000 persons living. The average annual number of deaths has been 50,000 less in the last than in the previous eight years. Doubtless many things have contributed to this grand result, and it is not possible to say how much is due to each of them ; but it would be unreasonable to doubt that the chief good influence has been in all the improved means for the care of health which recent years have produced. This is made nearly certain by the fact that the largest gains of life have been in the diminution of the deaths from fever, and of the deaths in children under 15 years old ; for these are the very classes on which good sanitary measures would have most influence.

The annual number of deaths from typhus, typhoid, and the unnamed fevers, has been about 11,000 less than it was about 20 years ago. The annual number of deaths of children under 5 years old has been about 22,000 less than it was ; and that of children between 5 and 15 has been upwards of 8,000 less.

These are large results, and though they tell of only deaths, yet they bear on the chief subject I have brought before you,—the working power of the nation ; for, however much of the diminished mortality of fever we might assign to improved methods of treatment, yet the diminished number of deaths means a very large diminution in the total number of cases. The deaths during the working years of life were 6,500 less ; and, this being so, we may hold that, if the average mortality was, say, 25 per cent., the diminution in the total number of cases must have been at least 25,000 ; and if we may believe, as before,

that each of these involved ten weeks of sickness, we have, in these fevers alone, a clear saving of 185,000 weeks' work in every year.

And so with the diminution of the mortality among children, there must have been a greater diminution in the number of costly and work-wasting illnesses, and a large saving of money that would otherwise have been sunk. And not only so: but many of the children saved in the last eight years will become bread-winners or care-takers; and who can tell what some of them will become? or what the world would have lost if it had lost them?

Let me add only one more reckoning. In a paper last year, at the Statistical Society, Mr. Noel Humphreys said 'that if the English death-rate should continue at the low average of the five years 1876-80, the mean duration of male life in this country would be increased by two years, and that of female life by no less than 3·4 years as compared with the English Life-table.' And he showed further that 'among males 70 per cent. and among females 65 per cent. of this increased life would be lived between the ages of 20 and 60 years, or during the most useful period.'

I should like to be able to tell the value in working power of such an addition to our lives. It is equal to an addition of more than 4 per cent. to the annual value of all the industry, mental and material, of the country.

But some will say,—admitting that it is desirable, though seeing how keen the struggle for maintenance already is—Can more than this be done? And the answer may be and must be, Much more. In this, as in every case of the kind, every fruit of knowledge brings us within reach of something more. While men are exercising the knowledge they possess, they may be always gaining better. This Exhibition has scores of things which are better helps to

national health than those of the same kind which we had 20 years ago, and with which the gains already made were won. If I were not in near official relation with the Jurors, I would name some of them : there are truly splendid works among them.

But do not let me seem to disparage the past in praising the present. It is difficult to speak with gratitude enough of what has been done, even though we may now see ways yet better.

Any one who has studied the sources of disease during the last 30 years can tell how and where it has been diminished. There is less from intemperance, less from immorality ; we have better, cheaper, and more various food ; far more and cheaper clothing ; far more and healthier recreations. We have, on the whole, better houses and better drains ; better water and air, and better ways of using them. The care and skill with which the sick are treated in hospitals, infirmaries, and, even, in private houses, are far greater than they were ; the improvement and extension of nursing are more than can be described ; the care which the rich bestow on the poor, whom they visit in their own homes, is every day saving health and life ; and, even more effectual than any of these, is the work done by the medical officers of health and all the sanitary authorities now active and influential in every part of the Kingdom.

But good as all this work has been, we may be sure it may become better. And the forces which have impelled it may still be relied on. We need not fear that charity will become cool, or philanthropy inactive, or that the hatred of evil will become indifference. Science will not cease to search for knowledge, or to make it useful when she can ; we shall not see less than we do now, and here, of the good results of enterprise and rivalry, or of the sense

of duty or of the sorrow for shame that there should be these evils in the land.

What more, then, it may be asked, is wanted? I answer, that which I have tried to stir; a larger and more practical recognition of the value and happiness of good national health; a wider study and practice of all the methods of promoting it; or, at least, a more ready and liberal help to those who are striving to promote it. In one sentence, we want the complete fulfilment of the design of this Exhibition, with all the means towards health and knowledge that are shown in it, and with its Handbooks, Lectures, Conferences, and the guidance which will be given by the verdicts of its Juries.

We want more ambition for renown in health. I should like to see a personal ambition for renown in health as keen as is that for bravery, or for beauty, or for success in our athletic games and field-sports. I wish there were such an ambition for the most perfect national health as there is for national renown in war, or in art or commerce. And let me end soon by briefly saying what I think such health should be.

I spoke of the pattern healthy man as one who can do his work vigorously wherever and whatever it may be. The union of strength with a comparative indifference to the external conditions of life, and a ready self-adjustment to their changes, is a distinctive characteristic of the best health. He should not be deemed thoroughly healthy who is made better or worse, more or less fit for work, by every change of weather or of food; nor he who, in order that he may do his work, is bound to exact rules of living. It is good to observe rules, and to many they are absolutely necessary; but it is better to need very few besides those of moderation and cleanliness, and, observing these, to be able and willing to live and work hard in the widest

variations of food, air, clothing, and all the other sustenances of life.

And this, which is a sign of the best personal health, is essential to the best national health. For in a great nation, distributed among its people, there should be powers suited to the greatest possible variety of work. No form or depth of knowledge should be beyond the attainment of some among them ; no art should be beyond its reach ; it should be excellent in every form of work, mental or muscular. And, that its various powers may have free exercise and influence in the world, it must have, besides, distributed among its people, abilities to live healthily wherever work must be or can be done.

Herein is the essential bond between health and education ; herein is one of the motives for the combination of the two within the purpose of this one Exhibition. I do not know whether health or knowledge contributes most to the prosperity of a nation ; but no nation can prosper which does not equally promote both : they should be deemed twin forces, for either of them without the other has only half the power for good that it should have.

It is said, whether as fact or fable, that the pursuit of science and of all the higher learning followed on the first exercise of the humanity which spared the lives of sick and weakly children ; for that these children being allowed to live, though unfit for war or self-maintenance, became thinkers and inventors. We may well observe the lesson, though science and art are not now dependent upon invalids ; minds are not the more free or the better now for having to work in feeble bodies ; each nation needs for its full international influence both health and knowledge, and such various and variable health, that there should be few places on earth or water in which some of its people cannot live, and multiply, and be prosperous.

If, therefore, we or any other people are to continue ambitious for the extension of that higher mental power of which we boast, or for the success of the bold spirit of enterprise with which we seek to replenish the earth and subdue it; if we desire that the lessons of Christianity and of true civilisation should be spread over the world, we must strive for an abundance of this national health, tough, pliant and elastic, ready and fit for any good work anywhere.

XX.

PERIOSTITIS FOLLOWING STRAINS.

From 'Studies of Old Case-books,' 1891.

SEVERAL cases have been published in which necrosis has occurred in a portion of a bone on which a straining muscle has pulled with unusual force either once or, in quick succession, many times.¹ But I think that the production of various less degrees and different forms of periostitis² by violent pulling has not been sufficiently considered. Such cases are, I believe, frequent; as, indeed, they well may be if we consider the enormous force of the strain often put on the periosteum, whether by the contraction of muscles whose tendons or shorter fibrous attachments are mingled with its structures, or by the pulling of ligaments stretched in violent wrenchings of joints. It must be, at least, advisable to remember always that, together with the mischiefs done in severe strains to ligaments and other proper structures of the joints, there may be serious injury done to the periosteum on the adjacent portions of the bones. Some of the consequences of strains may thus, I believe, be better understood and better dealt with.

I had a good example of necrosis produced by mus-

¹ Among the first and best are some by Mr. Solly, in the *Lancet*, November 3, 1855.

² I say 'periostitis' without meaning to imply that the periosteum alone is inflamed, and not the subjacent bone as well.

cular pulling in a young lady 16 years old, of rather weakly general health, who was overworked in exercises which required all the force which she could use with her right deltoid. Sharp inflammation about the shoulder followed, and then abscess over and behind the acromion; and when I let out the pus I could feel a part of the acromion bare. In a few weeks the dead end of this process separated and was easily removed, and then, without further trouble, all healed.

No cause of the necrosis could be suspected except the pullings by the muscle, and the general disturbance of health was only such as was explained by the acute local disease.

In another case, a gentleman, training for the Universities' boat-race, felt his habitually good health failing, and became unusually weak, and during the last few days before the race had a good deal of pain in his left upper arm. Still, he continued rowing hard every day, and he rowed very hard in the race. In the evening after it he had severe pain in the left elbow and upper arm and some swelling about the joint. A few days later, when he was first surgically examined, there were swelling and redness at the elbow-joint, especially over the olecranon and the condyles of the humerus. The joint evidently contained fluid, and was extremely painful when moved. The back of the arm, also, especially about the origins of the triceps, appeared intensely inflamed, and there was acute fever. Briefly, this was the beginning of acute deep inflammation of the whole upper arm with acute necrosis of portions of the wall of the shaft of the humerus, and suppuration of both the shoulder and the elbow-joints.

In the course of the year numerous sequestra, chiefly from parts of the humerus at and near the insertions of the triceps and biceps, were separated and removed. At

the end of the year I excised the still suppurating elbow-joint, and then all healed. The patient remains well after many years.

Among a few more cases illustrating the same points as these do, I find one of a weakly boy, with evidently scrofulous brothers, who strained the anterior muscles of his thigh in kicking hard at a slate. He kicked it only once; but acute necrosis of the femur followed, and quickly fatal pyæmia, and he died in little more than a week.

Next to necrosis, I believe that the most injurious effect of periostitis due to pulling is that which we see in some cases of caries. Cases are only too common in which caries of the bones of a hand or a foot, ensuing after slowly progressive periostitis, are ascribed to strains at the affected part. Scrofulous children's hands are pulled hard, and caries or chronic periostitis ensues in one or more metacarpal bones or phalanges; or they sprain a wrist or a foot, and similar results follow in the bones of the carpus or the tarsus. In cases such as these, when the force used is not in a blow or in pressure, the strain of pulling on the periosteum may fairly be deemed the only injury likely to be followed by such disease. The structures of the joints are, indeed, commonly involved; but this is by extension rather than by coincidence of inflammation.

I believe that caries of the lumbar or lower dorsal vertebræ, such as precedes angular curvature of the spine, may often be justly referred to similar injury of the periosteum when hard-pulled by the psoas, as in high-jumping or in preventing a backward fall. The progress of the disease may often be too slow to be sure as to its origin; but it was not so in the case of a man of 24, who was under my care at St. Bartholomew's.

Three weeks before his admission he had fallen with a heavy load on his back, and fell with his right leg twisted beneath him. He had had a great deal of pain at the right groin and had remained quiet since the accident; and on admission a firm painful swelling was felt above the outer half of the crural arch, well defined in front but not behind, and apparently filling the hollow of the ilium. And he could not completely straighten his thigh.

The swelling was the beginning of a large psoas abscess, which was opened and discharged a great quantity of pus. Its cavity soon after became full of blood: then was washed out and cleaned; and then the case went on after the usual manner of the time, near thirty years ago, and the patient died about five months after the accident, exhausted with suppuration, hectic, blocked femoral and iliac veins, and bed-sores.

The abscess extended through the whole space occupied by the iliacus and psoas muscles, and scarcely a trace of them remained. It extended upwards to the first lumbar vertebra, a portion of which was bare. There was a larger bare surface on the posterior part of the crest of the ilium, but no necrosis or evident ulceration of the surface of either bone. Besides, the abscess had opened through one of the bed-sores, and the cavity of the right hip-joint and the whole of the articular cartilage of the head of the femur had been destroyed by ulceration.

Cases such as these, proving so grave injury of periosteum and bone after strains, may be enough to make it sure that strains of less severity, or in healthier persons, may produce many degrees of less, yet not unimportant, damage. An excellent example was in an elderly gentleman who came with a well-marked circumscribed periosteal swelling—a node it might have been

called—on the inner border of his tibia, which had formed quickly after a severe night-cramp in the muscles of his calf. Much more common are the enlargements of the tubercle of the tibia which are often seen in young people much given to athletic games. They complain of aching pain at and about the part, especially during and after active exercise, and the tubercle may be felt enlarged and is often too warm. The pain often continues, more or less, for many months, and there may be enlargement of the bursa under the ligamentum patellæ, and the tubercle may remain too prominent; but common as are these cases, especially in our public schools, I have never known grave mischief ensue in any of them, and they get well of themselves. They may represent one of the least degrees of periostitis due to strain; the increase of the prominence of the bone is only just beyond that which may be deemed the normal limit for the attachment of vigorous muscles.

A yet less degree, I believe, is indicated by the pain and tenderness at the anterior edges of the outer condyle of the humerus which some feel after driving a 'pulling' horse. And similar to this is the share which the periosteum has in the 'lawn-tennis-elbow' and the less frequent 'fly-fisher's-elbow'; and in both of these the result of the periostitic part of the trouble is often seen in the uneven nodular edge of bone like that formed in common osteo-arthritis.

The large number of cases such as these which I have found in my case-books makes me think that 'sprains' ought to be thought of as more complicated injuries than they usually are. It seems common to think only of the stretching or rending of the ligaments and other structures on one side of the sprained joint; but besides this there are the crushing of those on the other side, and the hard

tugging of the muscles which strive to prevent the injury, and which in this strife pull hard on periosteum and bone. Such pulling may tear some part of the muscle itself, or it may be followed by long-abiding complete loss of power, or permanent wasting, or, I believe, by wide-spreading abscess; or it may rupture the tendon of the muscle, or separate it from its attachments. All these injuries due to the hard strain of muscles are well known; but the immense force which they indicate may, sometimes, tell most on the periosteum with which the fibrous structures fastening the muscle are mingled, and in which aching pain is apt to last very long.

I think that differences in the injuries done to one or another of these structures may often be associated with differences of age. In the elder, the tendons are apt to be weakened or partially wasted or softened, as one sees in the tendons of the biceps and the quadriceps, and in the Achilles-tendons; and these changes are often symmetrical. The periosteum, on the contrary, in advancing years becomes more tough and more inseparable from both bone and tendon. In the younger, the tendons are strong, they do not waste more than the muscles; but the periosteum is much less firmly fixed and is softer and more vascular than in later years. Hence, probably, it is that the periosteal troubles which I have described are much more common in the young, especially in the less robust young, than in the middle-aged and old.

So many of the injuries of which I have spoken occur in athletic sports, that I may be expected to write urgent protests, and even claims for some sort of legislation. I am not disposed to do anything of the kind. The advantages, both moral and muscular, of the free and self-managed games in our schools are immeasurably greater

than the disadvantages of the occasional damages done in them. And the pleasures of nearly all sports and the associated ambition for success in them are augmented by a consciousness of some danger. In schools, ordinary prudence on the part of masters playing with the boys, or looking-on, may suffice; especially if they are careful about those who are evidently weakly in their health. After school-life, men must be trusted for self-care in sports.

XXI.

ON SPINES SUSPECTED OF DEFORMITY.

From 'Studies of Old Case-books,' 1891.

AMONG the fears of disease for which one is consulted none is more frequent than that of lateral curvature of the spine. These fears are felt, especially, by mothers among the richer classes ; and usually the fear is only for their daughters' spines. It is thought essential to the welfare of a young lady that her spine should be straight, and her form not notably unsymmetrical, and that she should habitually sit upright with her back unsupported. There is no such thought for young gentlemen, and it appears to be, chiefly, a consequence of this difference that in the well-to-do classes lateral curvature of the spine is at least twenty times more frequent in girls than in boys. For mothers seldom look at their sons' spines ; and they let them sit with their elbows on the table, loll back in their chairs, and lie flat on their stomachs, and do many more such prudent things as in the daughters would be deemed shameful. Thus boys' spines grow straight ; the muscles helping to support them are not over-tired, or, when they are, they can be rested in any comfortable posture. But among girls the postures deemed graceful must be maintained till some deformity is discovered or suspected, and then the poor girls must be made miserable by the treatment deemed necessary for its cure.

The folly and the mischief of this contrast are happily becoming known : the good rule of letting girls grow up like boys is becoming more and more widely observed, and a larger proportion of them are well-formed, graceful, and strong.

Still, the unfounded fears of deformity of the spine are far too frequent, and they are maintained, in many instances, by the existence of slight deviations from the supposed pattern-shape which are quite harmless. It seems to be assumed by some that all spines should have curves and other characters exactly similar to those which are seen in artists' models or in anatomical plates. It is much more probable that there are as many varieties of healthy spines as of healthy chins or cheeks, or as many in the human species as in the horse or ox. Round shoulders, straight backs, hollow loins, and other such shapes are as common in members of the same family as are any other family features, and they are due to the imitation of parents in only the same degree as may be the expressions of similarly formed limbs or faces.

I wish I could describe the variations of shape as accurately as are the forms of spines studied and measured in skeletons or sections by such anatomists as Sir William Turner and Professor Cunningham. But this is not possible ; and yet the varieties of form may be well enough observed, even as those of faces or of any single feature may be, though they be not exactly measured.

I find notes of some of these varieties which may supply hints useful to be remembered when examining cases of 'Suspected Spines.' They relate to the dorsal and lumbar portions of the spine in which alone the feared lateral curvatures are usually looked for ; and they relate to none but young persons.

But, first, the suspicions are often founded on pains in the back which are, by some mothers, regarded as sure forerunners of deformity. When they are complained of, the whole length of the spine should always be examined, for they may be signs of disease in it, though, indeed, they rarely are so. The most common result of the examination is that the spine is found well formed, but tired through having been too much exerted in holding the body upright while sitting or standing. The tired muscles ache prudently, needing rest and more various activities.

I often quote for such cases, and for many more, an excellent sentence which I learned from Sir Henry Holland:—‘Standing has all the disadvantages of posture without any of the benefits of exercise.’ And for spines it may be said that sitting upright has similiar disadvantages without the benefits of rest.

Such aching muscles are, I think, most common in girls whose spines are very flexible; and this flexibility in all directions should always be tested. An apparent excess is harmless, unless it be associated with such muscular weakness that when the spine is extremely bent in one direction it can hardly be lifted up again. A very defective mobility is more suggestive of disease, especially if it be associated with pain at any definite part of the spine, and if the pain be increased in very wide movements or in violent and not guided movements, as in sneezing or laughing. Such pain with restraint of movement may be due to a rheumatic trouble, but it should always suggest a suspicion that there may be carious disease of the spine and a possibility of angular curvature. If careful examinations of this kind be always made it will be found that only a very small proportion of the aching backs of young or recently young persons are

associated with spinal deformity. Many of them, as I have said, are due to fatigue and the want of various exercise; many more may be called neurotic, and may be remedied by improvement of the general health. In this group are most of the cases in which the pain is described as intense or agonising; ¹ intense pain is extremely rare in young persons whose spines are really diseased.

Next to the cases of suspicion due to back-ache the most frequent are those due to awkwardness or carelessness in posture. Many, perhaps the majority, of cases of real deformity have their origin in wrong postures too long maintained, whether through carelessness or under compulsion. But, commonly, the imitation of deformity long precedes the reality, and may be known by the facility of at least a temporary remedy or concealment in a proper posture, especially in bending low. If a patient bending down, and nearly touching the ground with her hands without bending her knees, shows not only a rectilinear spine but similar muscular forms on each side of it, there should be no fear of deformed spine; there is none present, and there will be none unless through mismanagement.² Fitting exercises and well-proportioned restings will prevent any permanent deformity. They must vary according to the error in each case, but one general rule may be given, and it is good for many cases besides these, namely, that all exercises and habits, postures, games, drills, and the rest, should be such as may best conceal the defect; for such concealment is one of the best ways towards cure. The muscles which can conceal a defect

¹ I speak here only of cases in which 'back-ache' is referred erroneously to disease or deformity of the spine. It may depend on renal or ovarian or other internal disorders: but of these cases I do not venture to speak.

² If there be doubt when this test by stooping is applied, it is often useful to see the spine while the patient sits on the ground and then bends down.

are those which most need exercise, and which, being strengthened, may most help to a cure. Such concealment of defect may be hard for children and others like them; but with care even these may be taught, and for a very large number of drooping and tilted shoulders, and limp spines, and other such imitations of deformity, the cure by concealment is sufficient. It is thus that strict drill often teaches soldiers to conceal such defects as an unequal length of limbs, ill-made feet, partial stiffness of a joint, and by concealment to diminish some of them.

Of the cases in which deformity of the spine is feared on account of some more abiding unusual shape, some are associated with inequality in the lengths of the lower limbs. In the most marked of these the inequality is due to imperfect growth or other change after disease of one hip or knee, or after a temporary infantile paralysis; but some are only instances of a more than usual defect of symmetry. Exact equality in the length of the limbs is rare;¹ but if the inequality be not more than a quarter of an inch, it will hardly be observable at the spine. A greater inequality may so lift up the side of the pelvis above the longer limb that the flank will appear sunken in, and the lumbar part of the spine must be habitually, however slightly, curved, so as to hold the upper part of the body erect; or else the spine, though straight, will be oblique.

In all examinations of the spine for supposed or real curvature the lengths of the lower limbs should be looked to; not only because of the obliquities and even the curvatures wholly due to their inequality, but because such inequality may increase the degree of curvature

¹ Numerous careful measurements of limbs have been published by Dr. Garson, Dr. Cox, *American Journal of the Medical Sciences*, and others.

when it is real and chiefly due to other causes. The measurement may be made in many ways ; but anyone in the habit of exercising his sight in discerning exactness of levels, as in the hanging of horizontal bars or other such things, may measure the limbs accurately enough by placing his hands on the crests of the ilia while the patient stands erect and equally on both feet ; or it may be tested by making the patient sit on the ground on a low seat and bend forwards.

This inequality of the lower limbs is only one example of the very common deviations from exact lateral symmetry in even healthy and beautiful persons.¹ Such deviations may be almost always seen, if closely looked for, in the size and form of the two sides of the face, in the shoulders, the sternal ends of the clavicles, the sides of the neck, the breasts ; and some of these may suggest false suspicions of deformity. Especially may these arise when the two sides of the chest are evidently unequal. In some of these the difference is only an original defect of symmetry, and it often co-exists with similar difference in the two sides of the face, well marked in the eyebrows, or the lower borders of the jaw ; in some, and these more evident, the difference may be due to contraction of the chest after pleurisy or empyema. In cases of extreme inequality there may be well-marked dorsal curvature of the spine towards the larger side, and this is scarcely alterable unless by the gradual expansion of the contracted side of the chest. In less marked cases, such as are most of those which are due to original deviation from symmetry, the curvature is unimportant and may be remedied,

¹ It might be better to speak of deviations from lateral symmetry as being constant rather than 'very common,' but in such cases as I am describing I have in mind only those which may be evident to the naked eye. I doubt whether exact symmetry, such as might be deemed mathematically exact, can be found in any organic form.

in very young persons, by exercises giving wider movements to the arm on the smaller than to that on the larger side.

Together with these deviations of the spine, associated with naturally defective symmetry of the lower limbs, may be observed those associated with habitual resting on one limb more than on the other: whether because of weak ankles, or corns, or anything else. For all alike it has to be remembered that bad postures, long maintained, will bring in bad shapes, and even good postures maintained in weariness are hardly less injurious.

Many of the cases of inequality of the two sides of the chest are attended by that which some mothers regard as a sure sign of spinal deformity, an outstanding of the scapula on the larger side. But the fear is erroneous, and I think it may safely be held that no position of the scapula is alone a sufficient sign that there is spinal deformity. An unusual position of one or both must always make it right to examine the spine; but, if there be no unhealthy curvature, rotation, or other sign of deformity in the spine itself, the position of the scapula is not evidence of any. Their dissimilarity may be due to inequality of the two sides of the chest, or to one-handedness, and the associated inequality of growth of the two limbs, or to a deviation from symmetry in muscles or in bones, like that of any other pair of parts.

Among the varieties of shape in spines which may be regarded as personal, in the same sense as are the varieties of faces, or of hands or feet, I need not speak of any but those that suggest fears of deformity.

One of the most common is the too great curvature of the upper dorsal part of the spine, giving the owner the appearance of 'round shoulders' and of 'poking.' If it be a regularly arched curve, not angular, it is not to be

regarded as morbid; good drill and similar exercises may conceal and nearly cure it.

At the other and lower end of the dorsal part of the spine there may sometimes be found a defect, or as it were a delay, in the curving forward towards the lumbar concavity, and the hollow of the loins looks less both in length and in depth than it should be. But neither this nor the opposite peculiarity, in which the loin is extremely hollow, is the result of disease. The hollow loin is sometimes very short, sometimes long. In the former case it looks like a diseased sinking-in, the more so the fatter the patient is and the higher the rising of the fat on the nates. But neither of these varieties may be regarded as in any sense a deformity, unless all unusual faces are to be so called.¹ However much the anterior or the posterior curves of the spine may in these cases vary in length or depth—however long-loined or short-loined anyone may be—if there be no deflection to the right or left, and if there be no angular bending or local unreadiness to move, the patient may be ‘let be.’

I would not seem to imply the same in respect of cases of apparent bending forwards of any part of the cervical spine. It is sometimes seen at the beginning of caries, and had better be seriously suspected if it be associated with defective movement or unready and painful movement of the head.

And there are some appearances of lateral curvature and even of posterior projection which are not to be regarded as deformities. Some think, indeed, that the upper dorsal part of the spine has always, when well formed, a slight curve towards the right. Such a curve is not rare,

¹ They are examples of such varieties of form as Sir William Turner has shown may, probably, be found characteristic of different races of our species. — *Journal of Physiology*.

but it is far from constant, and appears to be sometimes imitated by a slight lateral deviation of the spinous processes, as if they were drawn over by the muscles or the weight of the stronger arm.

Instead of such a general deviation from the true median position of a part of the spine, there may sometimes be seen a slight lateral deviation of the spinous processes of one or two dorsal vertebræ. Whether it be due only to their own shape, or to any irregularity in the position of their vertebræ, or to a slight rotation of them, I cannot tell, but I have never known any harm associated with it, or with any similar condition of a lumbar or cervical vertebra.

In other cases, in which the whole length of the spine may be justly median, one, or sometimes two, of the lower dorsal or of the lumbar spinous processes are more prominent than they usually are, or than they would be in a model. They stand out from the uniform curve towards the hollow of the loin as if due to a partial or commencing posterior or angular curvature. The appearance is often deceptive, and should be tested by making the patient bend low down till the hands are near the ground. If this is painless and the spinal curve become uniform there need be no fear. But the examination should be very carefully made, and, if there be doubt, if the bending down be difficult, or really painful, or if the other movements of the spine, especially the curving backward, be restrained, and, still more, if the general health be impaired, or there be clear evidence of tuberculous inheritance, it is best to be suspicious that the projection may be due to caries or softening, and may be the beginning of angular curvature. And this is advisable in the case of adults as well as of young persons, for even in adults, as I have said in the next paper, it is not very rare to meet with instances of disease of one or two vertebræ or

intervertebral ligaments, slowly progressive and scarcely to be recognised till the spinous processes are prominent. Of course, help to diagnosis may sometimes be found in knowing how long the projection has existed; but the evidence on this point is usually doubtful.

More rarely one sees spines in which two or three of the spinous processes, far distant from each other, stand out prominently and singly. They give a strange look to the spine, but imply no more disease than a naturally ugly nose does. They are a good example of what I have already said, that in different persons healthy spines may be as unlike as any other of their features.

I am conscious that this paper may seem to encourage a temper of mind which is in some of us habitual, and I suppose is in all occasionally felt—the temper that disposes one to make too light of a case, and be ready to think and say ‘There’s nothing the matter.’ If I am in this risk of doing mischief I would try to prevent it by reminding the reader of the great difficulty of proving a negative, and that, as a general rule, it requires much more time and care in examining a patient to justify a decision that there is no disease than it does to ascertain what the disease is if there be any.

XXII.

OBSCURE CASES OF CARIES OF THE SPINE.

From 'Studies of Old Case-books,' 1891.

AMONG or in connection with these may be found instances of great difficulties of diagnosis, especially among cases which occur after puberty. On the one side are the cases of 'nervous mimicry,' in which all that are commonly supposed to be signs of disease of the spine are imitated with emphasis, but in which no organic disease is found. On the other side are cases, much more difficult and serious than those, in which grave spinal disease exists in unusual conditions, or with so little indication that it is likely to be completely overlooked. Of the first set of cases I have written in one of my 'Clinical Lectures' on Nervous Mimicry, and in the preceding paper on Suspected Spines; of the second I will here tell briefly one case in illustration of the difficulty of diagnosis in each of three groups. In the first of these are cases of caries of the spine beginning in persons more than fifty years old; that is, at an age when it is so rare that symptoms which, in early life, would be deemed characteristic are apt to be misunderstood. In the second group are cases of adults in whom the symptoms are vague, and the pains so misplaced that they are commonly referred to some disease deemed much more likely to occur than spinal caries. In the third group may be cases in which there is so little indication

of disease of any kind or anywhere that the whole process, including the angular curvature, is completed before anything is observed.

In the first of these groups, the case of which I had the best, though far from complete, knowledge occurred in a barrister, about fifty-five years old, a member of a family deemed delicate though they lived long. He had had weakly health, but had worked hard at the Bar and in Parliament, and had had no marked illness till in March 1858, when he appeared weakened by overwork. He had at this time a severe shivering fit, which was followed by some kind of fever, and then by increasing weakness. At the end of May he began to have pains at his back and sides, which were deemed rheumatic, and were carefully treated; but he became more feeble and lost weight, and still suffered pain and panted in the exertion of walking even a few steps. No intra-thoracic disease could be found, and an accurate diagnosis seemed impossible to Sir George Burrows and others, who at that time watched him.

After four months, in September, spinal disease was suspected, and he was advised to rest, recumbent; but he still continued to have almost constant pain, and it was felt as if shooting from the spine along the direction of the six lower ribs, and sometimes at the epigastrium. At the end of six months from the beginning of his illness his pain was more limited to the lower part of the dorsal portion of his spine, and now, when I first saw him, I found that he had a more than natural sensibility, and a feeling as of jarring, on pressure or striking of the spinous processes of the lower dorsal vertebræ. Two of these processes, also, were prominent and had a marked deviation to the right; they had previously been quite duly linear. He now moved his back, in whatever direction,

with extreme caution, holding it usually very straight ; his lower ribs, also, moved very little in respiration, and in any muscular exertion, even in much talking, he became nearly breathless and as if quite exhausted. There were no signs of pulmonary or any other internal disease, or of affection of the spinal marrow. But the diagnosis was now clear : disease, probably caries, of some of the lower dorsal vertebræ.

He was kept absolutely at rest, and took iodide of iron and such good food as he could digest ; and issues were made and kept open by the sides of the most painful part of his spine. Thus treated, his condition slowly improved. In about four months he could sit without pain, and his breathing power had increased, and with gradually diminishing pain and increasing power, he was in about thirteen months from the beginning of his illness able to leave town, and to walk short distances without distress. The prominence and lateral deviation of the spinous processes continued, but became less abrupt, the adjacent vertebræ above and below having gradually adjusted themselves to the change.

He regained health, and was at eighty strong and active, above the average of men of that age. He died when eighty-one. His case was certainly one of ulcerative disease of the spine, whether in the bodies of some of the dorsal vertebræ or in some of the intervertebral cartilages. Few cases could be much more difficult of diagnosis than it must have been at its beginning.

The other cases of this kind which I have seen were in two men between fifty and sixty. In both the disease was in the lowest dorsal vertebræ ; in both it was, at first, scarcely indicated ; in both it ended with angular projection of the vertebral spines. One of these cases was

believed to have followed a 'fever'; both recovered with nothing evidently worse than the slight deformity.

One of the best examples in the second group of cases was in a surgeon about twenty-eight years old, who had lost two brothers in phthisis, and had generally had delicate health, but had been in active country-practice for five years. He believed that his disease began eighteen months before he consulted me, and that it was due to his having, for several months, ridden a fast, rough-going horse. The chief signs had been pain and weariness between his shoulders and in his flanks, and slight general loss of strength. These were much increased by walking far, and so much by riding that he had been obliged of late to give it up entirely, and to do his work driving with cushions at his back. During these eighteen months, but at what period was not exactly known, the mid-dorsal portion of his spine had acquired an angular curvature; the seventh or eighth spinous process having become very distinctly prominent, and those above and below it rather less so. There neither was when I saw him, nor had been, any pain or tenderness at the place of the curve or on any other portion of the spine; but he had some aching or feeling of constraint in it when he used his arms very actively. Besides the angular curvature of the spine, there was at the same part a slight lateral deviation. In his general health there was nothing plainly wrong except the loss of strength.

Dr. Baly and I advised him to give up work, and live very quietly on the coast for two or three months, and take iodide of iron. It seemed certain that the case was one of caries of some of the vertebræ, slowly destroying their substance or that of the intervertebral cartilages.

At the end of three months he told me that he had

followed the advice given him and felt well enough for all his work. Two years later he was still in active practice, and the projecting portion of his spine appeared rather less sharply prominent, but his general health was feebler, he was sooner tired, and sometimes had the old aching between the shoulders or pain in his intercostal muscles. Still he was fit for work, and so he continued for three years more, making nearly seven years from the beginning of his illness ; but now he began to have a bronze colour of the face and scrotum, and rapid loss of weight and power, and other signs of disease of the renal capsules, and with these he died.

The diagnosis of this case before the change of the shape of the spine would have been very difficult ; and the difficulty was still greater in three otherwise similar cases which I have seen. In these the pain near the diseased, but not yet deformed, part of the spine was so acute, not in the spine itself, but at one or both sides of the chest, that it was thought the patient had some strange kind of pleurisy. The angular projection becoming evident explained the pain. But in all of them the disease appeared more limited than it usually is in children and young persons ; for the angular curvature was formed by only one vertebra becoming prominent, and the deformity was not noticeable unless they were undressed. The recovery appeared for a time complete : but some had recurring pains ; and of the three cases of which I heard the ends, one, I was told, died with disease of the renal capsules, and two with pulmonary tuberculosis.

The best case that I have seen of the third set, in which the caries of the spine passes through its whole course unobserved, was in a gentleman who consulted me in July 1859. He was between twenty and twenty-five,

very stout and strong, addicted to the most athletic exercises, to hard drinking, and often to sexual excess. Except in being over-fat, which he ascribed to his being out of training, he considered himself in perfect health, and capable of the most violent exercise; he was ready for any violence in anything. He had no sign of general illness, and I could find no organ in fault, unless in so far as his digestive organs were disturbed by his intemperance. But he had well-marked angular curvature of his spine, involving about the last three dorsal vertebræ; a curvature characteristic of carious disease. His lower ribs moved very little, and his breathing was chiefly diaphragmatic and sternal; but of this and of any history of disease he knew nothing. He knew that he had always been hollow-loined, but he was sure that this angular deformity had not existed more than three weeks. He was not aware of its forming; it was noticed by mere chance; and even now no pain could be felt in it, and it did not seem to affect his health in any way.

I told him that if the deformity were really so recent it would imply very dangerous disease, and that he had better give up his present manner of living; but he was doubtful whether he would.

A month later he again came to me and said he had been living quietly, with only occasional violence in exercise; but he was now less sure of the deformity being recent. He thought it might have been only unobserved, and he remembered that for three years he had been subject to frequent pains along the course of his lower ribs on both sides, and to sensations of sinking and weakness in the lower part of his chest. These pains had been very irregular in their occurrence, and were generally increased by fatigue, and often by violent horse-exercise. They had been thought rheumatic, neuralgic,

or pleuritic; but they had never prevented him from hunting, or any other amusement that he wished for, and he had never laid-by for a day on their account.

Believing that the disease was thus shown to be of at least three years' standing, and was now not in progress, and feeling sure that he would not observe any rigid rules of treatment, I advised him to avoid hard riding, railways, and all jarrings of his spine, and to lie down after exercise and live in all ways carefully. He would not follow even this advice, and I heard that he went-on leading a thoroughly dissolute life, suffering, at times, with symptoms of disease in the spinal marrow, and gradually losing power in his lower limbs. He did not become paraplegic, and the deformity did not increase, but in the winter of 1861 he became phthisical, and he died in 1862.

It may, with some reason, be said that this is hardly a fair case to cite in evidence of the overlooking of the progress of spinal disease, and that a less reckless patient would have given and asked for more attention to his pain. I could have told a case in which, in similar unobserved progress, the pain was trivial; but this which I have related may be the more notable because of such an absence of all the other usual signs of the disease that the patient was never once obliged to spend a day quietly.

And the case is a good instance of that which I have referred to in another note, namely, the effects of the contrast between the neglect and overlooking of pain by a reckless athletic fellow such as this was, and the watchful outlook for pain by any sensitive woman who suspects that she has a diseased spine. In the one, the real disease goes on unobserved, uncared for, rarely felt; in the other the mimicry of disease is with constant suffering: in the one, life is enjoyed, though, it may be, after a vile fashion,

even while disease is going on to death ; in the other, life is made miserable by that which will not shorten it.

In the third volume of the Clinical Society's 'Transactions' and in my 'Clinical Lectures and Essays,' I published a paper on what I called 'Quiet Necrosis,' giving cases in which necrosis of bone, such as is usually attended with many and well-marked signs, went on through a long course unobserved. I see the name sometimes used by others describing similar cases, and the examples of caries of the spine contained in the last paper induce me to point out that 'quiet diseases' are much more frequent in surgical practice than seems to be generally supposed. Everyone must have observed the various intensities of the symptoms in different examples of what must be called the same disease ; and in some the usual symptoms are either absent or so little marked that they may be unobserved even by those who are watchful. Especially among cases of diseases of joints I have known several of these. The most marked was in a lad of whom, though he had been very carefully brought-up, it was never known that his left shoulder was completely stiff till he went to Eton and was found defective in some of the school-games. The joint was immovable, the muscles round it wasted, but it was free from all signs of active disease, and, I fully believe, always had been so ; and, whatever had been the disease, it was now past. I remember two similar cases in which, in children of ten or twelve years old, always well cared for, the hip-joint became stiff without any evident sign of disease beyond the increasing local discomfort. Both these were such as might be deemed scrofulous, and it might fairly be believed that in both of them scrofulous disease of the hip-joint went on 'quietly' till it ceased

when the joint became stiff. The same quietude may sometimes be observed in osteo-arthritis (chronic rheumatic arthritis) of the hip-joint in elderly persons. The limb becomes shorter and the joint stiff without any other discomfort than may be referred to these two changes, or, perhaps, with some pain in the knee which withdraws attention from the real seat of disease.

Other cases might be mentioned. They would help to show that it is useful to keep in one's mind the name of 'quiet disease.'

XXIII.

ERRORS IN THE CHRONOMETRY OF LIFE.

From 'Studies of Old Case-books,' 1891.

I HAVE often found reason to believe that, in the study of pathology, we do not sufficiently consider how much of disease or of its variations may be due to disturbance of the time-rate at which the organic processes should be discharged. And I think this may be because too little thought is bestowed on the law that all the processes of organic life are regulated as strictly in regard to the time in which their work is to be done as they are in regard to the quantity of material employed in them or the size and shape of any structure formed by them. We are too apt to forget that there is a Chronometry in Life as well as a Chemistry or a Mechanics.

I venture, therefore, to reprint a condensed report of a lecture which I gave at the Royal Institution some thirty years ago (on the Chronometry of Life). It may make some more ready to study the errors of chronometry in disease if they are in the habit of thinking of the exactness with which rates of time are, in all living creatures, observed in health.

*[This reprinted lecture on the Chronometry of Life (1859)
is omitted here.]*

Before speaking of the errors of chronometry in relation to disease, I wish to point to the probability that in many of the instances which I cited of the observance of time in disease, the time-rates may be determined chiefly by the events in the lives of microbes, which were scarcely subjects of study when the lecture was given.

And I may suggest an inquiry, which I have often wished that I could pursue, into the influence of the variability of a time-rate, as of one of the conditions on which may have depended the 'selection of species.' The subject was hardly within the general range of study thirty years ago.

It is not difficult to imagine many terrestrial events in which the 'survival of the fittest' among species or individuals might depend, chiefly, on the possibility of adjustment of a time-rate in one or more processes. In the ordinary course of nature the time-rates in each creature are adjusted to those of the external conditions most necessary to its life. The development and hatching of the young pigeon keep time with the development of its parent's crop; the development of the mammalian foetus with that of the mother's mammary glands. Such adapted changes in parents for the sake of their offspring are everywhere to be seen; and so are the instances in which the developments of animals of all kinds keep time with those of the plants or other animals on which they live.

It is hardly too much to say that the whole course of nature among living things thus depends on their mutual adjustment in time as well as in all the other conditions of their lives. And if this be so, then any alteration of the time-rate of life in any species might gravely affect its fitness for an unchanging environment, or might lead

to the natural selection of a few of the species whose differences from the rest would be transmitted to some of their offspring. Or, in the other direction, a considerable change in the time-rates of any of those things—such as a vegetable or other food—on which a species depends for its continuance, might lead to the extinction of that species, unless the time-rates of some of its members and their offspring could be adjusted to the change. It cannot reasonably be supposed that the time-rate is the only characteristic of the processes of life which is not liable to variation; the ‘late’ and the ‘early’ varieties of a species are instances of its occurrence; and whenever a change of time-rate occurs there must arise an opportunity for ‘natural selection,’ just as there does on the occurrence of any variation in the form of a limb or of any other part.

But I can only wish that I could work at this: it is too far beyond the range of both my knowledge and my opportunities. I commend it to younger and more learned students of evolution.

This subject of chronometry in life has, naturally, been very often in my mind, and facts illustrating it have often seemed as if they might be arranged in some systematic manner useful for the evidence of general principles in physiology. Through want of ability or want of time I have been unable to do this; but I may mention one instance in which the time observed in processes in living structures may show a general principle which, I think, can hardly otherwise be established, namely, that none of what we may regard as the component parts of any organised structure are exactly alike. For example, no two or more minute structures can appear more exactly alike than do the adjoining cells of a leaf; yet, if we watch them in the unfolding

or other expansion of the leaf, they do not all at the same time attain the same size or the same colour; and yet more evidently in their decay, we see the yellow or other tint gradually extending in different shades of intensity from one part to another of the same leaf. The external conditions of the whole leaf may be the same: if all or any of its cells were exactly alike, they would pass through changes of colour in exactly the same time; but they do not so change unless, perhaps, it be the cells placed in exact symmetry on two parts of the leaf. Now, the fact that different lengths or periods of time are required for producing similar changes in two or more structures, is sure evidence that those structures are not exactly alike. I know no other test than this that could detect a difference among, say, the cells most near to one another.

Similar illustrations may be found in so many structures—for instance, in two parts of the same tooth, the same hair, the same cornea—that I think the rule may be deemed general and constant; and it may be useful to bear it in mind as evidence of differences among structures in which no difference can be detected by even the finest chemical or physical test yet within our power. It is often said that no two cases of disease are alike; we cannot expect them to be so if no two elementary structures in the same person are.

The errors of chronometry—unpunctualities in life, as they may be called—may be studied in two different, though often mingled, forms. In one, an unusual time-rate is maintained in all parts, or, at least, in all important parts, but is maintained alike and uniformly in all; in the other the time-rate is erroneous in only one or a few parts. To use an illustration from what may happen in

an orchestra, in the one case the error is like that of music which is played too fast or too slow, but in which all the musicians keep the same time; the music is not good, but there is no discord. In the other case it is like the music in which one or more of the musicians play too fast or too slow, and are as constant sources of discord as if their instruments were out of tune.

The most frequent and obvious instances of the first class in which the time-rate is erroneous, but is uniform in all parts, may be observed in the earlier and in the later periods of life. It is matter of common observation that some young people look older and some look younger than they are, and the former are generally deemed stronger and healthier than the latter. In later life, also, some people look older and some look younger than they are; but the judgment is reversed, and the latter are deemed stronger and healthier than the former. The judgments are so often right that one is often prone to think that looks are better evidence of the probabilities of life than are the registers of births; for a register can only tell the time since birth, but looks, i.e., the appearance not only of the features in the face but of the size, shape, and manner—can often indicate accurately the rate at which the vital processes have been carried-on, and at which they may be moving to their end.

But, although this may be generally true, there are very many exceptions to the rule, and it may be that in this as in all pathology, more truth may issue from the study of exceptions than from the ready acceptance of general rules.

The young persons who look older than they are, and who have had in all their structures more than the mean time-rate of development and growth, are usually healthy and potent in resistance of disease, but exceptions are not

rare; scrofula may be seen well marked in some whose stature, muscular power, and sexual development are three or four years in advance of the mean at their registered ages. Still, if there be no evident marks of defective health, they who, when they are less than twenty, look older than they are, may be deemed healthy above the average.

I think that the greater time-rate does not, in this class, continue in later life; that they are not commonly among those who at all ages, especially when in or near old age, look older than they are. But this needs more accurate inquiry.

On the other hand a large majority of those young persons who look much younger than they are, are weakly and have what is called delicate health. Some are evidently scrofulous, having clear signs of such diseases of the glands, or joints, or other parts; and some are as evidently syphilitic by inheritance, or rickety; and many more, though not plainly thus diseased, are certainly prone to scrofula, tuberculosis, acute rheumatism, and other diseases of this class. They have large tonsils, granular pharynx, swollen nasal mucous membrane, and other minor signs of such diseases. Thus, together with their chronometric errors, they have coincident or, perhaps, in some measure, consequent errors of structure or composition in this or that part or group of parts.

It may seem probable that the 'delicacy' of the young who look younger is generally due to this qualitative error rather than to the error in their time-rate. For in a numerous minority of them there is no such delicacy; all their functions are discharged in right mutual adjustment, and in quantity befitting their apparent, though not their registered, age; and after what may seem a long time they acquire a manly, robust health and full power,

though rarely a full stature. Still, it is very often useful when elderly or old people are ill, especially if they are ill with obscure chronic disease, to ask them about their looks in early life. Facts helping to a right diagnosis may often be thus learned; many of the subjects of senile scrofula will tell that they were small and delicate while young, though not evidently unhealthy, and that after their youth they were hearty and strong till their old age began. They may be examples of a second childishness in bodily though not in mental defect.

I have often tried, but often in vain, to learn the conditions which have retarded the time-rate of development and growth in the young. I have not found that the health, good or bad, of parents has any clear influence on the time-rate of their children in early life, nor that the children of the old are different in this respect from those of the younger. Yet various time-rates are often characteristic of families, and examples may easily be found of two or more families in the same conditions of life, and equally good general health, in which the young people of one family look much younger than those of the same ages in the other, and are really younger in all reckonings but that of the times since birth. I have notes of an example of eight children of the same family living to middle age, small and always looking young, and with one exception always healthy. Their parents, this exceptional one told me, 'looked their age.'

The most frequent cause of delayed growth, when any cause for it can be assigned, is an acute illness, such as scarlet fever, diphtheria, or whooping cough. Mr. Galton¹ relates a striking instance of retarded growth

¹ *Inquiries into Human Faculty*, p. 235. I have heard also from Mr. Galton that, in the growth of cereal crops, a check at any period of their progress is followed by long-continued delay in growth, imitating the consequence of an acute illness.

of children's heads during and for a time after each of their occasional illnesses. A less considerable fact is in the checked growth of the nails during illness.¹ Mr. Galton's fact was ascertained by very careful measurements. Often, though less accurately, one sees that after apparently complete recovery from scarlet fever, pneumonia, whooping cough, or other acute illness in early life, growth is so retarded that two, three, or more years seem to have been lost, and young people of fifteen or eighteen look, and are, as if not more than twelve or fourteen years old. They may be, for their apparent age, quite healthy, but more often they are not, and they are among those who will prove the value of a general rule in both practice and education, that both lessons and the rules of living should be adjusted to the apparent rather than to the registered age.

It is often said that during and after acute illness some young people increase in height very rapidly. This may be true, but I think that such growth in height is seldom, if ever, associated with equivalent growth and development of organs whose size is more important than is the length of the lower limbs and spine. Length thus attained is not significant of high development;² it may no more be so than is that strange increase of fatness sometimes occurring in young people after illness. I have seen this especially in some when convalescent after

¹ Dr. Wilkes has related a case in which this occurred in the sea-sickness of two voyages across the Atlantic of only seven or eight days each.

² I say 'thus attained' because more generally stature and development may seem in some measure correlative. Mr. Roberts (*Report of the Anthropological Committee of the British Association*, 1883) reports that, 'Compared with the general [adult] population, lunatics show a deficiency of stature of 1·96 inch, and of weight of 10·3 pounds, and criminals of 2·06 inches and 17·8 pounds.' Nationality also must here be taken into account. 'In this country shortness and lightness are deemed signs of degeneracy; in America and the older Australian colonies excess of stature is so regarded.'—Roberts, *St. George's Hospital Reports*, vol. viii. 1874-6.

acute scrofulous joint-disease; they have become big, round, fat, and dull, yet they looked too young.

What is true of these general illnesses in respect of the general time-rate may be true of the influence of local disease or severe injury on the time-rate of the one part affected: it may be made or may become comparatively old or young; almost certainly it will be out of time, as well as out of harmony, in structure. And, as all parts do not grow at the same rate at all times, so various disproportions may be determined in different persons by the time in early life in which any general illness may have occurred.

A slow time-rate is often observed in children who were prematurely born. They seem behind time till their growth is nearly complete, but some of these continue to grow longer than usual, and some attain, in time, great stature.

The foregoing facts relate chiefly to delayed growth, and it is, of course, in early life that such chronometric errors are most frequent; but delays, or slow time-rates, may also be noted in later years. Then, the looking young, in which is often expressed the fact of being comparatively young in all but years, is probably due to slowness in the degenerative processes of life; and we may fairly guess that this may also explain the many instances of great longevity in persons who, when they are very old, say that they never were strong, never vigorous in health. Their whole time-rate has been slow, and they have, therefore, lived long; as among several clocks of similar construction, those which go slowest will go longest.

The contrast with these may be found in some who look prematurely old; but it is hard to judge of these, because so few have lived long without having some

disease or disturbance of this or that part which has destroyed the uniformity of the time-rate at which all the organs began to live, and which may have been necessary to healthy life. Hence it is that natural death—that is, death through uniform and exactly contemporary decay of all the structures—is very rare. The few cases in which it is observed do not prove more than that there is no fixed time at which it should occur.

The foregoing facts, incomplete as they are, may be enough to show that important and useful knowledge may be gained by a careful study of those deviations from the usual time-rates of life in which all parts deviate alike, the rate being in all parts equally too fast or too slow. A still larger field for study is in what may be deemed the premature development or degeneration of one or more parts; the rate of life being not uniform in all. But on this I can scarcely do more than recommend it for the study of younger men, for I have no clear facts concerning it in relation to any of the most important parts.

The best illustrations are in some of the least important parts, but these may suffice as suggestions for seeking similar facts in the most important.

Premature development is only too commonly seen in the teeth. Their time-rate may be faster than that of the jaws, with which it ought to coincide exactly; and in this case they come out crowded, and the more so the more exactly they keep time with one another. Often, however, they do not keep time either with one another or with the jaws; they come out both crowded and disorderly. I have seen a young gentleman who, at eighteen, had cut all his wisdom-teeth, and his teeth were in advance of everything, for his jaws were small and had not room for them; he had no appearance of hair on his

face, and he was altogether boyish in build and mind. The opposite error in time, shown in delayed tooth-cutting, is nearly as common, and cases are not rare in which the eruption of the wisdom-teeth, which signalises their complete development, is delayed till middle age or later.

Errors in time such as these are very troublesome, not gravely important. But surely it is very probable that similar errors occur in other more important parts. Premature puberty may be cited ; and it may at least be suggested that grave trouble may ensue if, for example, a heart should be acquiring its full power more quickly than the lungs or arteries. And let it be remembered that even the smallest defect in chronometry, beginning in early life and continued, will become constantly more serious in its effects ; just as an inaccurate clock becomes more wrong the longer it goes.

The premature degeneration of this or that part, whether with mere wasting or with fatty or other change of structure, while the other parts go-on at their usual time-rate, is, no doubt, often due to disease of the part ; but it is not always so. The changes which are natural in old age, and which when uniform end in natural death, often begin at different times in different parts and make unequal progress. Here again the plainest illustrations may be found in the teeth and in the hair. We are apt to think lightly of premature grey hairs because they are common and may be seen in many who are in good general health ; but it had better be believed that analogous untimely degenerations may occur, without disease, in much more important parts. All parts do not often grow old at the same rate ; and as people of the same age according to the register are, in many instances, of widely different ages by every other test, so is it with the several

organs of the same body. For all the purposes of life some parts may have grown old much faster than others; some may even be dying as of old age while others are still fit for active life; and these dying parts may never have been the seats of any morbid process; their errors may have been only chronometric; they have not kept time; they have too quickly become 'grey.'

I think that we may thus explain, or, at least, may thus express, some of what are deemed diseases of old age. One sees people with scarcely a grey hair at seventy whose stomachs are simply incapable of digesting, as feeble and as quiet as in those who are dying of old age. And so, one sees some with shuffling tottering gait, and with such weak and shrivelled limbs, as might be deemed natural in people of ninety, and who yet say that they can eat and drink and digest as well as ever. Similar illustrations of different time-rates observed in different organs in the same person may be found, I suspect, in some weak dilated hearts, and some wasted emphysematous lungs and inelastic arteries. Certainly, I think we may thus explain many cases of distended urinary bladders in men of sixty or but little older, who in general health show no signs of old age. Their bladders become distended with gradually accumulated urine, which they are too weak to discharge, though there be no hindrance in the urethra or prostate. The patients, themselves, are often unaware of the distension, the bladder being alike insensible and inactive; and the urine may be quite healthy, unless, as in some cases, it be of very low specific gravity. Similarly, the rectum may be found weak, dilated, and irritable, with detained solid fæces, in elderly people who are sturdy and active, and in whom all other organs seem naturally strong. The rectum alone in them is old-aged.

I venture to think that our pathology and, sometimes,

our practice would be improved if local defects of working power were more often thought of as errors in the time-rate of life in the defective parts; if we would think of the 'age' of each part as not always wholly or exactly expressed by the time that has elapsed since it was first formed; if we would bear in mind that any internal organ may deviate from the general time-rate of the body as much as the teeth, and the hair, and the skin, which we so commonly think of as old before their time. We should not be content with asking a patient how old he is; we should ask ourselves, how old is his heart, or his brain, or any other part which seems now less healthy than the rest.

XXIV.

THE USE OF THE WILL FOR HEALTH.

From 'Studies of Old Case-books,' 1891.

It may often be observed that not only the signs of some diseases but their progress and issue may, in some measure, be determined by the patient's will; I mean not only by the will to live prudently or unwisely, but by the direct influence of the will on sensation and motion.

The subject may be studied in, at least, three sets of cases; some show how the will, by acts of attention, can affect the clearness and intensity of pain and other morbid sensations; others how it can control the movements of muscles generally involuntary; others how it can, at least in some degree, determine the methods of some of the processes of organic life. And in every set of cases illustrations may be found of the power of the purposely-educated will to prevent or remedy the defects due to its natural or permitted weakness.

INFLUENCE OF THE WILL IN PAIN AND OTHER
SENSATIONS OF DISEASES.

We have no means of estimating the degrees of pain from similar sources in different persons, or from different sources in the same person. But we may fairly speak of different diseases as more or less painful, and of different persons as feeling some less and some more acutely the pain of this or that disease. On the whole, too, we may

be sure that the more cultivated races are far more sensitive to pain than are the less cultivated; and that, among ourselves, a similar rule holds; that mental cultivation increases the sensibility to pain as well as to pleasures; and that it does this, in great part, by increasing the force and facility with which the will, though it may be almost unconsciously, can direct attention to external things. Moreover, what is, in this respect, true for pain is true for other morbid sensations, itching, burning, tingling, and the like; but pain, as generally understood, will give the best illustrations of my subject.

The influence of the attention, that is of the will directed to sensations, is illustrated by facts at opposite extremes; by those which, at one extreme, show that pain is not felt when the attention is completely distracted, and, at the other, that pain becomes more keen and more constant when the attention is constantly, though it may be almost unconsciously, directed to it. Among the former are the numerous instances in which soldiers engaged in battle have been for a time unconscious of having received severe wounds. The latter are shown in cases, to which my notes often refer, of persons who have unwittingly cultivated a natural sensibility to pain. These show that it is with pain as it is with sight or touch or the muscular sense, or, for example, with hearing. As some persons have, naturally, an 'ear for music,' so have some, and often the same persons, a very fine sensibility to pain; and, as one who has habitually and very earnestly directed his ear, *i.e.*, his auditory nerve-centre, to the discernment of sounds becomes, in time, keenly, and without conscious effort, sensitive to even the least sound or the least variation of a tone, so is it with some in regard to pain. With almost constant direction of the mind they increase every pain,

and even find or insert pain in places and conditions in which one less exercised would not feel any. And this goes on till they become able, without conscious effort, to observe the least deviations from natural sensation and almost wholly unable to distract the mind from them.

Hence it comes that, in a given kind and amount of injury or disease, that which one person hardly observes or regards as a mere discomfort, is to another an 'agony'; and this, it may be, not in imagination, not in invention, but as really felt as are those refined sensations of the artist, the musician, or the gourmet, to which common people cannot attain. I think that nearly every one in practice must have seen cases illustrating these statements: but I may tell of some for examples.

Thus, as showing insensibility when pain was not expected or attended to: A boy from whom a part of a finger was to be amputated refused to let it be done without chloroform; but, as it was not convenient to give this, the surgeon signalled to a dresser to pretend to give it with a wet sponge. The boy smelt the sponge till he believed himself insensible, and the operation swiftly done was not felt.

And, for the indifference to pain of the uncultivated mind: A lady in New Zealand gave a pair of leather boots to a naked-footed native boy. They were too narrow for his toes, and he at once, with a tomahawk, chopped a piece off the inner border of the end of each of his great toes, covered the wounded parts with some dry stuff that lay on the ground, put his feet back into the boots and wore them till the wounds were healed.¹

¹ It is often said that 'savages' have exceedingly acute senses because they hear or see some things which others cannot. But this acuteness is only in relation to one or a few things; just as dogs have an exceedingly acute sense of smell for game or blood or some such thing, but none, so far as we know, for flowers or wines, or the many more which are discerned

And for an instance of the inducing of pain by attention. A lady showed me a cancer of the breast which I could be sure had existed for several months, but which she had not been aware of till, some days before she consulted me, she saw it in her looking-glass. I asked if it had been painful, and she answered, 'Of course it has been painful ever since I saw it.' It had not been so till her attention was directed to it.

From facts such as these, numerous as they are, we may deduce some rules to be observed in practice :—

(1) It is neither wise nor just to say that all these 'painful' people, as they may be called, only imagine the pain which they describe. They really feel it all, unless they wilfully tell lies, and they are unable to distinguish the pain which may naturally be due to the injury or disease from the augmentation of it which is due to their too watchful attention.

But (2) in these, and, in various degrees, in many other persons the pain suffered supplies no sure evidence of the kind, quantity, or intensity of a disease. The value of the evidence of pain cannot be estimated without some knowledge of each patient's mental character, and, when he or she is known to be naturally very sensitive or very watchful, it will often be safe to hold that, in obscure cases which have long endured, without any grave injury to the general health or any manifest change of structure or loss of weight, the more the pain the less is the organic disease, if, indeed, there be any at all, at the seat of pain.

(3) On the other hand, they that are careless and not by cultivated men. In this wide and various range of keen sensibility in all the sense-organs the evolution of our race is shown as evidently as in the range and variety of sentiments and intellectual power. In our estimate of the degrees of sensibility of any sense-organ in ourselves and other animals we may fairly reckon the range and variety of power as well as the intensity.

observant of themselves, however keen they may be in their study of external things, may be found with diseases long overlooked or neglected, or only sometimes felt, even though they be diseases commonly very painful. Here, too, the diagnosis must be determined by the surgeon's sensations rather than by the patient's. He may have in his mind, 'It is by what I feel, not by what you feel, that this question is to be settled.'

(4) A lesson for all whose sense is quick for pain is that they should strive for such control of will as to be able to divert the attention, as much and as often as possible, from the watching of pain. For pain expected, watched for, long thought of, or talked of, will come; it will come in or from the nerve-centre and may be as bitter as any from the nerves' ends. Any real pain that is often described by one who feels it is thereby nurtured, and the power of discerning it is being made stronger; and, conversely, the longer and the more often the attention can be diverted from any pain, the less does the power of discerning that pain become, just as the muscular or any other sense, when out of practice, loses some of its cunning. And patients should not trust to others for this distraction; they should educate their own wills so as to be able to direct their attention to whatever may for the time be best.

INFLUENCE OF THE WILL ON OTHER THAN THE VOLUNTARY MUSCLES.

The influence of the will on disorderly or inconvenient muscular actions may best and most usefully be studied in the half-involuntary organs, whose action is usually preceded by distinct sensations and associated with an effort of the will.

The rectum and the bladder are the best of these.

Usually, in healthy persons, they have certain habits in their acting ; the once a day for the one, the three or four times a day for the other, and these at certain regular periods of the day or night. The habit is generally a good one : but I think it is advisable that men should not in this more than in anything else become the slaves of habit ; for if they do, then, when the habit is perforce disturbed, it may bring distress, and to some persons utter misery and alarm. Cases of retention of urine are only too common in those who, being in the regular habit of passing it every three or four hours, are by some accident obliged to hold it for six or eight. Or, if they escape this trouble once or twice, then they are frightened lest the difficulty should occur again, and they are always watching their bladders, and are on the alert for the sensation of fulness and of the muscular disquietude of the walls ; the sensation which, like all others, will come the quicker and the sharper the more earnestly it is expected.

Healthy people should not allow this. These half-involuntary organs are good servants but very bad masters ; and the more they are indulged the more peremptory do they become. They should be educated to subserviency ; and the earlier in adult life that their education is begun the better it will be. If they are fairly healthy, any one with a resolute will may easily teach them. It is sufficient if the sensation that usually indicates the need of action be sometimes neglected ; if the attention be diverted from it, or permission to act be resolutely refused, or else be promised in half an hour or an hour. It is well, at least for some, if this education be insisted on, at first, only at times and in places in which it is sure that the promise to permit the action can be fulfilled. A bladder imperfectly educated should not have to claim such a fulfilment in a railway-carriage or a church. But with a little prudence

of this kind I believe that any healthy and not too old person may educate his bladder to be quiet for eight or ten hours, and within these limits to act at any time that is most convenient to him. Similarly, any one may teach a healthy rectum, and may thus gain a very desirable liberty for business or for pleasure at the times that are most convenient to himself or others. It involves nothing more than a resolute extension of the usual range of the will.

It is needless to multiply the instances in which one has seen the influence of the will in tendencies to disorderly muscular action. A strong-willed man can often repress the words and movements significant of delirium; or he can conceal such a degree of drunkenness as a weak man would show in every word and movement; and, unless in real disease, he can enjoy a wider range of liberty in respect of food and drink and sleep, of work and of rest, than is possible for one who is timid, self-watchful or irresolute, although, in all that is commonly understood as bodily health, they may be alike.

I feel strongly that this influence of the will upon the health should be a subject of resolute and careful self-education. Men should resolve that they will not allow every unusual sensation to distract their attention from work or any other important object. If they would steadfastly go-on working in spite of noises in the street or of talking in their rooms, they would more easily be indifferent to sensations suggestive of disease. The opposite of this is too often allowed and even boasted of. It is common to hear healthy people say, and sometimes as if it were praiseworthy, that they cannot sleep on this side or that, or without a pillow or some such help. In some of these there may be just reason for their defect,

but in the greater number it is a mere habit grown out of a want of the will to resist some discomfort. If such people would resolutely pass some hours, or a night or two, without sleep, sheer fatigue would ensure them sleep in any posture whatever. And others, many of whom are healthy, tell that, unless they have their meals at regular and fixed times, they cannot digest them. There are few of these who may not safely resolve that they will not fix the times, but will breakfast and dine whenever they please or when circumstances make it most convenient. Their digestive organs will submit to their wills, if their wills do not submit to them.

These small imperfections should be, if possible, amended by the will, because they all restrict that reasonable and useful liberty of life which we may enjoy, and they diminish our power for the work which we ought to do; they are hindrances to both pleasure and duty. Besides, harmless as they may seem in health, they may be serious complications in disease. A man, for instance, who cannot sleep on his right side is at a great disadvantage if he have a pleurisy on the left; he who cannot sleep on his back is doubly distressed by a fractured thigh, till, after some sleepless nights, the necessity of lying on his back enables him to sleep on it as well as he used to do on his side; he who binds himself to certain times for his meals is unfit for the casual and irregular work by which others can prosper.

I may as well say that I am the more sure of these things because I have myself done what I have here recommended, and have thus worked hard for more than fifty years, happily and without harm. And I have learned to believe that many persons, or all who are not definitely unhealthy, may go even beyond these rules. If they will resolve that their stomachs *shall* tolerate this

or that food, or shall not have this or that stimulant, whether wine or mustard or any other, their stomachs will, in time, yield. They need not be more resolute than most of those must be who wish to be smokers; these find that, after a few days' distress, pleasure comes from the very thing that was a cause of misery. They have only to place themselves willingly in difficulties such as others have to endure whether they will or no; as when, for pleasure or for duty, they go to sea or to places where the food seems at first unfit to eat. In time they learn to enjoy it and it serves for active work.

Elasticity and pliancy in all the functions of the body are among the essential characteristics of health, and all should strive to attain them. That is not completely good health which cannot endure any disturbance from the usual habits of life. I wish there were as much ambition for really good health as there is for athletic strength or bravery. There is too much of the very opposite, and one hears people boast that they cannot do this or that, and they write or talk as if all others ought to be like them. It is as if a cripple should insist that others ought to limp as he does.

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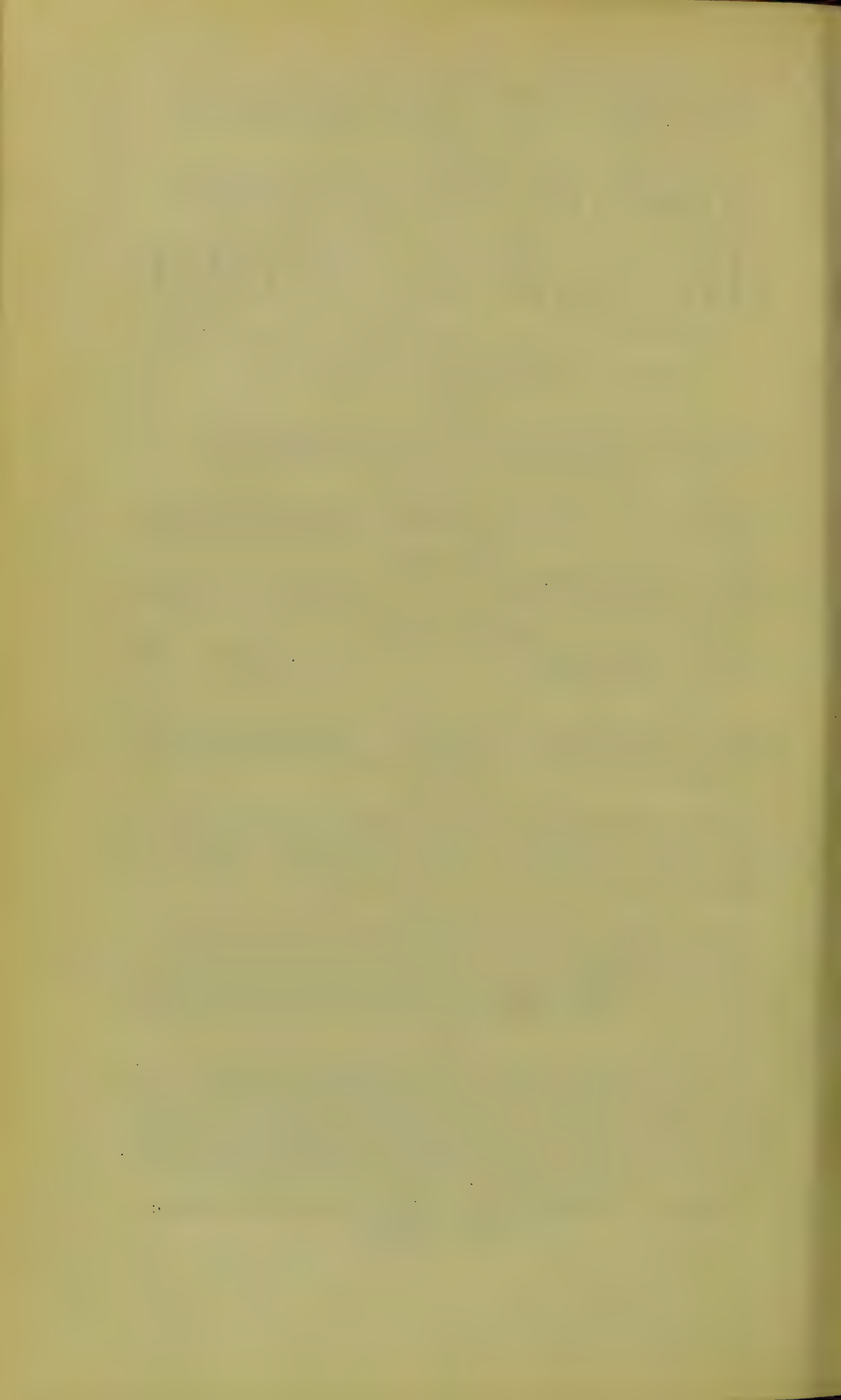
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